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THE PSYCHOLOGICAL REVIEW

THE ORGANISMIC HYPOTHESIS AND DIFFERENTIATION OF BEHAVIOR

II. THE REFLEX ARC CONCEPT

BY ORVIS C. IRWIN

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Not only are the traditional cell and neurone theories instances of biological atomism, but the reflex arc, likewise, particularly in physiology, has been a logical instrument employing the same type of thinking. However, there is a considerable body of evidence which is not amenable to a consistent and rigorous interpretation by the reflex chain view of behavior, and which calls for a radical modification of that view. Some of the more important researches which point to an organismic formulation of the problem are those of Child (4), Coghill (5), Langworthy (8, 9, 10), Lashley (11, 12, 13, 14, 15), Tracy (20), as well as Minkowski's (16) work on the reactions of the human fetus and recent findings on the behavior of the newborn infant.

Child (4) formulated an organismic view of the reflex arc as well as of the origin of the nervous system from his investigations on the physiological gradient. By showing that both arise from the more fundamental processes in protoplasmic gradients he has cut the ground from under an atomistic treatment of reflex. His position is stated in the following paragraph.

"It is a self-evident fact that the reflex arcs and the reflex behavior of any organism are dependent upon the course of development in the organism. They are consequences and expressions of all that has gone on before. The receptor and

effector connections of each reflex arc, the interrelations of different arcs, whatever their adaptive evolutionary significance, must all have a physiological basis in the development processes and are evidently outgrowths of the general organismic pattern. In fact, the physiological continuity in the individual between the physiological or metabolic gradient and the reflex arc is evident. The physiological gradient is the general physiological foundation on which the reflex arc develops. If we consider development in its functional, rather than in its structural aspects, it appears that the gradient is the primitive and generalized excitation arc out of which the various reflex arcs develop" (4, p. 234).

The principle involved here is that before the phylogenetic appearance of reflexes organisms react on the basis of non-neural patterns. Moreover, it also involves the origination of reflexes and neural patterns from the preneural patterns. There is some evidence to show that such is the case in the development of the individual. If this is true, then it is a difficult task for the atomistic theory to explain how the discrete reflex, which comes relatively late in ontogeny, can be the elemental principle which accounts for behavior.

Coghill's (5) painstaking analysis of the neural anatomy and the behavior of the amphibian *Amblystoma* affords a striking demonstration of the origin of reflex arcs from the preneural integration of the organism. He attempted with remarkable success to trace the course of development of behavior in *Amblystoma* and to correlate neural maturation with the appearance of new behavior patterns. These patterns in the order of their appearance are designated as follows: the non-motile stage, when the muscles can be excited to contraction by direct stimulation but do not receive impulse from nerves; the early flexure stage, in which head bending is the first response to cutaneous stimulation; the coil stage, when the organism responds to stimulation by coiling itself up; the 'S' reaction, which involves a reversal before complete coiling. It should be noted that these are massive types of activities involving the whole structure of the organism. It is a unified total reaction of the animal.

In the next stages of development segmental movements appear. These are movements of the gills, the fore-limbs, and the hind-limbs. It is this appearance of segmental activities subsequent to the primary massive behavior which is of such great significance for the theoretical interpretation of behavior not only in *Amblystoma* but, as will be seen later, in infants also. The following description by Coghill (**5**) will make clear the relationship between segmental and massive movements in *Amblystoma*.

"A day or two ordinarily elapses between the time when the arm begins to move with the action of the trunk before it acquires the ability to respond to a local stimulus without the perceptible action of the trunk. Such independence of limb action appears to be acquired by a gradual reduction of the trunk. . . . It is obvious, therefore, that the first limb movement is an integral part of the total reaction of the animal, and that it is only later that the limb acquires an individuality of its own in behavior. The local reflex of the arm is not a primary or an elementary behavior pattern of the limb. It is secondary and derived from the total pattern by a process of individuation" (**5**, p. 18).

It is quite evident that the crucial problem for any organismic theory of behavior is a concrete description of what is meant by 'process of individuation.' Coghill has demonstrated experimentally what the process of individuation of segmental movements out of the massive activities of *Amblystoma* actually is. "In our explanation of the nature of the movements of the trunk it was explained that nerve cells are arranged in a longitudinal series in the spinal cord in such a way that they conduct excitations from the head tailward, and that from these nerve cells side branches go to the muscle segments to excite them to contraction. But these side branches, as motor nerves, do not stop growing when they reach the muscle segments. Having attached themselves to the muscle segment and established control over it, they, by other branches, grow on beneath and beyond the muscle segment and invade the territory of the limb. The first motor nerve fibers to reach the muscles of the limb

are, therefore, branches of the same nerve fibers that stimulate the trunk muscles to action. Furthermore, these nerves reach into the territory of the limb-muscles long before muscle tissue is formed. As a result of this precocious invasion of limb-forming tissue by branches of nerve cells that are already integrating the trunk, the earliest movements of the limbs are of necessity totally integrated with trunk action" (5, p. 22).

It is plain then, from this description, how the local reflexes in the forelimb of *Amblystoma* arise by a process of individuation from the primary massive activity. This is a fundamental piece of observation.

Tracy's (21) work in the tactile reactions of the larvæ of the toadfish clearly indicates that the segmental reflexes are not the primitive components of behavior. These larvæ, like *Amblystoma*, exhibit motility before the specific activities of the limbs appear. At the time of the first movements the motor nervous system is developed but the afferent system is still largely undifferentiated. In other words, Tracy's work (21) confirms Child (4) and Coghill (5) in the view that the reflex mechanisms appear later than the earliest stages of activity.

The point is that the efferent nervous system functions before the afferent and commissural systems have developed. The work of these three investigators is fatal to the older reflex view. They have demonstrated that there is a stage when the efferent or primitive nervous system responds to internal stimuli, and that during this stage the organism does not respond to external stimuli. Now the generally accepted view has been that a reflex is a chain of neurones, an afferent connected with an efferent. However, before this condition obtains the organism will react; that is to say, activities proceed although the reflex arcs are not yet established. It is clear that these researches force a modification of an elementaristic reflex view.

Tracy (21) has, moreover, shown that the spontaneous activity in toadfish under homogeneous environmental conditions depends on internal metabolic processes, very probably

CO_2 tension. Brown (1) evoked progression movements in the limbs of fetal cats by means of asphyxiation and called attention to the internal or organismic factor in behavior.

Tracy (21) has summarized his conclusion in the following paragraph.

"There are two components of behavior, namely endogenous and exogenous activity. Endogenous activity constitutes the fundamental feature of body motility (progression) and is conditioned by internal (physiological) adjustments in connection with metabolism. Exogenous activity is oriented activity; it appears to be essentially the modification of the endogenous activity which results either from the stimuli which the organism meets during the excursions in the environment, or from those aroused by changes in external energy relations. The segmental reflex is not primary, but is the last type of activity to develop, and does not involve progression movements. It is probably dependent on specialized mechanisms differentiated apart from the primitive motor pathway" (21, p. 317).

Langworthy (8, 9) has conducted extensive studies on the correlation of activities in kittens and opossums with the development of myelinization of neural tracts. He finds that it is a general rule for the axones of motor cells and motor nuclei to become medullated before the axones and nuclei of the afferent system. In this respect his findings confirm the work on *Amblystoma* and the toadfish.

The well-known experiments by Lashley (11, 12, 13, 14) on the effect of cortical destruction upon behavior in rats and primates present strikingly a body of facts which find no adequate explanation in the reflex view of behavior. These experiments need not be cited here in detail. In general, Lashley's method was to destroy various areas in the cortex, the corpus striatum, the caudate nucleus, and the spinal cord, and then by means of simple mazes to test learning functions. He found that following each type of destruction rats could traverse the mazes without significant errors, although in some cases there might be serious postural disturbances. Cameron (2) repeated Lashley's experiments on cerebral

destruction, testing the rats in much more complicated mazes, and found that rats with frontal lobe destruction could learn new maze problems, were able to retain to a certain degree old maze habits, and exhibited adaptability to modifications of old habits. However, in contrast to Lashley's similar experiments Cameron found distinct inferiority on the part of the operated rats as against the performance of normal animals. Nevertheless, his operated rats did learn. The results of these experiments on cerebral extirpation point to the view that behavior does not depend on components localized in specific areas of the brain, but that the nervous system is a complex dynamic organization which operates as a whole. Lashley and Ball have stated the implications of these experiments for the reflex theory as follows: ". . . simple reflexes elaborated by a combination in chain reflex arcs have proved of little value for an understanding of the more intricate problems of psychology" (**15**, p. 98).

Swensen (**18**, **19**) has pointed out that the reactions of fetal rats resemble those of *Amblystoma*.

Behavior of the Human Fetus and Newborn

The remarkable observations by the Swiss embryologist Minkowski (**16**) present added difficulties for a consistent reflex chain view of behavior. Minkowski studied the reactions of twenty-three human fetuses ranging in age from about two months to about six months and varying in length from five to thirty-two centimeters. The fetuses were removed from the mother by Caesarian section and immersed in a saline bath maintained at 40° C. Because the postoperative physiological conditions are altered by the disruption of placental circulation, progressive asphyxiation occurs and death follows. Minkowski nowhere definitely states how long reactions can be observed under these conditions, but one is led to infer that it varied from a few minutes to perhaps an hour. Since these significant observations hitherto have not been available in English the results will be described here at some length. The reactions observed included movements of the head, trunk, limbs,

thorax, responses to cutaneous stimulation including the plantar and patellar reactions, mouth and jaw movements, proprioceptive and labyrinthine reflexes. The description of these movements will be reproduced in his own words as translated by the writer.

"Most of the fetuses observed made more or less vigorous movements of the head, trunk and extremities immediately after extraction. The head is turned or twisted to one side or the other, or it may be raised or lowered. The trunk is bent and straightened, the limbs flexed and extended, adducted and abducted, rotated inward or outward. These movements are slow, asymmetrical, arhythmical, uncoördinated, and they have a limited locomotor effect. They may extend to one joint or more, upon proximal or distal segments, upon one member or simultaneously upon several members."

In regard to thoracic movements he reports, "Only with the oldest fetuses could I observe movements of the thorax. In these cases extensions of the thorax were repeated from time to time and simultaneously the head would be thrown back. In one case of a fetus 20 cm. in length, I observed a repeated opening and closing of the mouth accompanied by arm movements. The duration of these movements was very brief, they continued scarcely a few minutes and more often could not be observed longer than one-half to one minute" (**16**, p. 723).

In six older fetuses ". . . respiratory movements of an unmistakable rhythmic character appeared both before and after cutting the umbilical cord. In one case, a fetus 30 cm. in length, during the course of an hour made one or two respiratory movements at intervals even when the mouth and nostrils were submerged in the saline bath. A careful recording revealed that these movements occurred regularly at intervals of one and one-fourth minutes" (**16**, p. 754).

However, it is Minkowski's descriptions of the response of the fetus to cutaneous stimulation that are particularly significant for an organismic interpretation of human behavior. "While spontaneous movements continue, and even after they have ceased, one can evoke various motor reactions

by rubbing, by pressure, or by the light touch of a brush. Usually, and this holds true of the youngest individuals (2–3 mo. cm.), there are no isolated reflexes which are localized in the segments stimulated, for movements spread to the remaining segments, eventually to the trunk and the head. Thus reactions are variable and irradiate over the whole body. For instance, light pressure on the foot will evoke flexion in the stimulated limb and also crossed reactions in the other limb. Moreover, it will also release activity in both arms as well as in the trunk and head. Likewise, touching a hand will result in reactions in diverse segments of the body. In one case, a fetus of 13.5 cm., I observed not only a flexion of both arms but, in addition, repeated opening and closing of the mouth and retraction of the head. The following generalization, therefore, will hold: *each skin area may serve as a reflexogenic zone for various reactions which have a tendency to irradiate more or less over the entire fetal organism.*

"With older fetuses movements resulting from stimulation are often more constant and less general in that they are restricted to the stimulated member. Nevertheless, the tendency to irradiate to other extremities holds in most cases" (**16**, p. 723).

Concerning the labyrinthine reflexes in fetuses, Minkowski reiterates his observations of the lack of preciseness and constancy of movement. "Like all fetal movements these are also extraordinarily variable and display either flexion or extension or adduction and abduction of both arms and legs" (**16**, p. 724).

These interesting and novel observations on the movements of the human fetus may be summarized in the following statements: (1) they are slow, asymmetrical, arhythmical, and uncoördinated; (2) constancy of response to a stimulus is absent; (3) the diffuse massive type of response in younger fetuses becomes replaced in older fetuses only in part by specific reactions to stimuli. Thus the rule of the behavior of the human fetus is what may be called 'mass activity.' This is greater during the early period of gestation and apparently diminishes somewhat in the later period.

Minkowski (16) has made equally important observations on the neural side. He has performed a series of operations on the spinal cord and brain of several fetuses. On transection of the midbrain he found no changes in the pattern of fetal behavior. The reactions exhibited the same generalized diffused, massive features characteristic of the intact fetus. The neuroblasts in the ventral horns of the cord were better developed and more highly differentiated than those in the posterior. These findings are consistent with those of Coghill (5) on the larvæ of *Amblystoma* and of Tracy (20) on *Opsanus tau*. That is, in all three forms the sensory and commissural nervous components develop after the motor cells of the nervous system. This neural condition very probably explains the behavior which has been termed 'mass activity.'

Langworthy (8, 9, 10) has studied the development of myelinization of nerve tracts in a seventh-month human fetus and finds that the order of medullation follows that of kittens and opossums. Langworthy's extensive work on kittens, rabbits, guinea pigs, marsupials, and on human material is consistent throughout in its results with the work of Coghill (5), Swensen (18, 19) and Minkowski (16).

It is seen, then, that studies on the behavior and neural development of the early stages of the growth of *Amblystoma*, the cat, the kitten, the opossum and observations on the human fetus all indicate that the reflex arc account of behavior needs modification.

During the course of my own observations on the reactions of newborn infants I found it increasingly difficult to use the reflex chain theory to explain infant behavior satisfactorily. The reactions of the newborn [1] are altogether too variable and too massive to be described in the rigid terms of that theory.

It will be recalled that Minkowski (16) found that the movements of the human fetus under six months of age are slow, asymmetrical, arhythmical, and uncoördinated. The

[1] The period of the newborn is taken to be the lying-in period, usually ten days to two weeks.

activity of the day-old infant, likewise, may be described in these terms. All of Minkowski's adjectives apply. However, after the post-natal feeding routine has become established (about the second or third day), the asymmetrical, arhythmical, and uncoördinated movements no longer remain slow. Quite the contrary, under experimental conditions without incumbrance of clothing and with all external conditions maintained constant, the activities increase in rate so that in two or three days after birth and increasingly thereafter, the experimenter experiences great difficulty in recording in shorthand these infant responses. At the end of ten days the movements proceed at such speed that a qualitative recording at times is utterly beyond the capacities of the experimenter and recording of an automatic quantitative type by appropriate apparatus must be relied on. At this stage isolated segmental movements of the infant, such as clenching of the fists, slashing of arms, kicking of legs, movements of flexion and extension, etc., have become lost in a massive activity which involves the whole infant organism. Literally everything seems to be going at once. The movements of extremities to a certain degree are a part of the movements of the trunk, reminiscent of the coil stage in *Amblystoma*. The observations regarding irradiation, diffusiveness, and variability in the human fetus hold for the activity of the human newborn. It seems to be true in the newborn infant that when movements are slow they involve separate or isolated segments. But when segmental movements increase in rate they eventuate in an irradiation of activity until the entire infant is strenuously at work. The trunk is thrown about, curved, flexed, extended, arched, and rolled from side to side. The movements of arms and legs merge with those of the trunk with great variability, being slashed and kicked, flexed and extended, adducted and abducted incessantly. Ankle, wrist, elbows, knees, fingers, and toes as well as facial muscles are involved as part of the larger movements. Crying usually accompanies them also. There are no regularities, no fine and precise gradations, little specificity in the reflex sense, and few effective coördinations

in this activity. To designate the type of reaction involving the whole organism, whether proceeding at a high or at a low rate, I have used the expression 'mass activity'. It stands in striking contrast to the movements of single segments of the infant. The latter type of reaction is called specific. The term 'mass activity' was suggested by Riddock's similar phrase 'mass reflex'. The modification is made to apply to the facts of the behavior of the newborn. This behavior is not reflexive in the conventional sense of the term, for the reactions show altogether too much diffusion and variability to stimuli to be called reflexive. The neural implications of this behavior have been pointed out elsewhere (**6**).

Not only does the reflex theory break down in the face of this type of behavior, but attention should be called to another fact. The enormous amount of activity observed in these newborn infants proceeded under external conditions in which light, temperature, sound, etc., were maintained as nearly as possible at constant. It is not presuming too much to suggest that this behavior is due largely to internal factors. This is an important point in the organismic view. The organism itself furnishes factors which affect behavior. This has been repeatedly demonstrated in experimental embryology and biology. Tracy (**21**) found it to be true in the toadfish, and Brown (**1**) in fetal cats. Carlson and Ginsberg (**3**), using rubber balloons inserted into the stomachs of two prematurely born pups, measured hunger contractions. They used this technique with two infants, one nine hours old, and one nine days old. In the case of the latter infant, they show a record of stomach contraction taken three hours after feeding. The writer has observed in newborn infants that this is the time when mass activity regularly is at its maximum. Although the balloon method was not used, activity records of infants throughout the first ten days exhibit a regularity of this phenomenon which leads to the inference that hunger contractions are an important part of the internal component of behavior, an inference for which Carlson and Ginsberg's kymograph tracings give objective evidence.

It is interesting to note that both Coghill (5) and Minkowski (16), using embryonic materials of animal forms at different evolutionary stages, found the same relation between massive activity and the specificity of the reflex. Coghill (5) demonstrated that the reflexes are later individuations of general body movement. Minkowski indicated that although some degree of specificity is present, the rule is that the younger the fetuses, the more diffuse are the reactions, and that with all fetuses the tendency to irradiation of movement is always present. It is to be expected, then, that by the time of birth specificity would be increased, but that a large proportion of movement would be of the nature of mass activity. It is this commingling of a rudimentary specificity of segmental movements together with considerable mass activity, which has been observed in the Ohio State studies on infants during the first ten days.

Some valuable observations on the newborn by the Shermans (17) bear out the point that specificity is a later rather than an early feature of the behavior development of the human young. In the case of eye coördination, using ninety-six infants, they showed that specificity of response to a slowly moving light could be measured by counting the number of coördinations. These increased rapidly from 37.5 per cent at two and a half hours after birth up to ten hours and then more slowly up to thirty-four hours when the number of coördinations amounted to 77 per cent. The Shermans made similar observations on defense movements in newborn infants. The movements were elicited by pressure on the infant's chin by the finger of the experimenter. "Diffuse defense reactions of the hands were observed in the youngest children. These movements consisted of throwing the hands about in an irregular manner, striking the offensive finger occasionally, but only by chance, . . . no infant below twenty-one hours of age made a successful defense movement. As the infant increased in age, the defense movements became less and less indefinite and irregular, and the pushing away of the examiner's finger a more coördinate response, up to the age of about one hundred and eight hours, when the

reaction was made regularly and accurately with both hands, the infant in many cases using one hand with good precision" (17, p. 66).

Differentiation and specificity of behavior in infants, therefore, probably develops out of an original matrix of mass activity. Mass activity probably is the earliest activity of the uterine organism. Segmental movements are aspects of it which become partly specialized in the uterine environment and are evident in the newborn. Nevertheless, at birth there is a great deal of nonspecific behavior. During the first ten days, as the Shermans show, specificity is progressing although, as has been demonstrated elsewhere (7), mass activity is the more striking phenomenon. Specificity in the sense that the same stimulus evokes the same response, as is demanded on the reflex view, develops much later than the period of the newborn. It is suggested that if the reflex comes relatively late in the uterine and postnatal development of the organism as specialization of generalized functions which are already present, it is difficult to understand how it can be an elementary unit on which nervous function and the integration of behavior are built. And this is a fundamental fact which the reflex arc concept is unable to explain. It would seem, then, that the physiological reflex is an inadequate basis for a defensible explanation of infant behavior. It is further suggested that the early mass activity, with its lack of mature specificity and the presence of so few effective coördinations or reaction patterns, probably points to an organismic rather than to a reflex chain theory of behavior.

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THE CATEGORIES OF SUBSTANCE, CAUSE AND FUNCTION IN FREUD'S PSYCHOLOGY[1]

By CARL M. WHITE
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The primary motive behind the psychoanalytic movement has been practical. Psychoanalysis arose as a means of bridging a gap in medical technique. It has been defined by its founder as "a method of treating nervous patients medically" (**6**, p. 1).

While the science of psychology is not directly interested in the art of healing, certain instructive results may issue from an examination of the manner in which this gap was bridged. There are three general constructions which may be placed upon the meaning of a neurotic symptom—a physiological, a biological, and a psychological construction. Psychoanalysis has adopted the psychological. We may pass in brief review these constructions, and thereby bring the singularity of psychoanalytic procedure, which is said to have justified itself by clinical success, into clearer relief.

In order to keep his profession on solid ground, the reputable physician has, throughout the greater part of the history of medicine, entertained a certain bias for physiological interpretations. Accordingly, a symptom of disease meant a definite organic lesion. If no such lesion was in evidence, one of two things could be said; either the patient's illness was 'imaginary,' or knowledge of the human body had not advanced far enough to enable medical science to diagnose the difficulty. Beyond this verdict there was no appeal until biology gained a footing.

With the increase of biological information and theorization in the latter half of the nineteenth century, an organ or function came to be regarded as a sort of index finger pointing backward toward a phylogenetic purpose which it had pre-

[1] Written under the direction of Professor Madison Bentley.

sumably served in the process of evolution. Meaning became closely associated, therefore, with the teleological notion of 'adaptation' or 'survival.' This construction was in time adopted more or less generally by psychiatry, and is well known to us through the works of Ed. Claparède, Meyer Solomon, Adolph Meyer, W. H. R. Rivers and many others. Claparède's language is typical when he says, concerning hysteria, "We shall, then, have to ask ourselves, *what is the significance of this hysterical distraction?* and whether it does not represent a reaction of defense which had its origin in adaptation" (4). The distinctive contribution of biology seems thus to pertain to this reference beyond: a symptom means an adaptive reaction to the environment.

Freud, the founder of psychoanalysis, accepts neither of these objective interpretations. His approach is in a sense opposite to that of traditional medicine in that he ignores physiology. As Stanley Hall remarks, in his preface to Freud's *General Introduction to Psychoanalysis*, "In one thing Freud agrees with the introspectionists, viz., in deliberately neglecting the 'physiological factor' and building on purely psychological foundations, although for Freud psychology is mainly unconscious, while for the introspectionists it is pure consciousness" (6, p. vii). In his masterpiece, *The Interpretation of Dreams*, Freud himself says, "We shall entirely ignore the fact that the psychic apparatus with which we are here dealing is also familiar to us as an anatomical specimen, and we shall carefully avoid the temptation to determine the psychic locality in any way anatomically. We shall remain on psychological ground . . ." (7, pp. 424–425). Freud's motive for this neglect appears to be furnished by the character of subject matter with which he is dealing. As regards neurotic symptoms, "there are either no discoverable corresponding changes of the anatomical organ of the soul, or else the changes are of such a nature as to yield no enlightenment" (6, p. 6). And while the rapid increase of physiological knowledge will doubtless elucidate, as time goes on, our understanding of the mechanisms of these disorders, we are told that therapy cannot postpone its practical work till that time arrives.

Again, the biological interpretation is said to be barren. It has undisputed value within limits, and to that extent psychiatry and psychoanalysis supplement each other, psychoanalysis supplying the omitted psychological foundation of psychiatry (**6**, pp. 6–7, 209–220). But construction in terms of the biological category of objectivity tends to distract our attention from the matter—important for psychoanalysis—of the psychological operations which make the content of a given symptom what it is at a given moment. Those who uphold the biological standpoint may or may not be correct in considering the dream, with Meyer Solomon for example, as a grade of psychological activity corresponding to that point in the phylogenetic scale to which the sleeper has, according to the degree of his relaxation, temporarily regressed (cf. **17**). Regardless of its tenability, such a theory fails to tell us anything of importance about the way in which (to put the matter popularly) dreams work. The theory looks on the dream from the outside, and merely takes note of what it points toward. It never takes us—to use a type of figure familiar to the reader of Freud—inside the dream so that we may arrive empirically at such modes of operation as 'condensation,' 'displacement,' 'dramatization,' and 'secondary elaboration.'

When the attempt is made to overcome by speculation the sterility of the biological category, Freud's critics lapse into the very error which they deplore in psychoanalysis. Thus one writer (**13**) tells us that all mental phenomena, to be understood, must be explained in terms of stimulus and response. In order to explain the meaning of the unconscious, we must inquire, 'What is the reaction within the cell?' This secret is held by 'cell phylogeny,' which confides the information that the primitive cell possessed what may be called 'reflex consciousness'—*i.e.*, the ability to react to external stimuli. But, in time, this primitive cell went inside a bony cavity where its apparatus and function became more complex. While it retained its reflex character by developing an axon for the purpose of keeping in contact with the environment, it also took on the higher faculty of

acting by itself. 'These cells do not have to be told what to do, *they know what to do.*' Sometimes the cells express themselves as 'conscious consciousness'; at other times, as 'reflex consciousness.' We are to mean by the unconscious, therefore, simply reflex action.

But this speculative glimpse 'within the cell' goes wide of the mark of elucidating what Freud calls the 'inner workings of the mind.' Such a disquisition is, in fact, not so much designed to tell us anything new about these workings as to translate their meaning into another category. The article is at once a striking example of the success that attends the imposition of an unnatural category upon a given body of data and of the obtuseness to the objective category of biology of certain psychoanalytic material.

We may take the case of the obsession mentioned by Freud in his *General Introduction* (**6**) to illustrate the manner in which this material yields itself to a psychological interpretation. The symptom is a senseless idea: a woman gives credence to an accusation, admittedly groundless, against the fidelity of her husband. The obduracy of the symptom brooks no denial of its importance. Subjectively it has destroyed the patient's happiness; objectively it threatens to break up a home. A physical examination proves wholly negative; and while we learn from the case history that this woman comes from a family in which similar phenomena have occurred, this information is inadequate for purposes of either diagnosis or prognosis. As Freud points out, even if we could truthfully state that all obsessions may be referred to degenerates, we should still lack the information we most need, namely, how this particular obsession differs from others. But as soon as we examine the symptom in the light of its psychological setting, we begin to discover clues to its meaning. (Freud calls this the 'historical method' of interpreting the symptom. Cf. **6**, pp. 221–235.) Apparently, the jealous woman herself had provoked the accusation because of a secret attachment to her portly son-in-law. The phantasy of her husband's infidelity was but an abortive means of reorganizing her relationships, at the expense of reality, to suit her own taste.

Freud's procedure from this point is, in effect, as follows. We may legitimately expect that this psychological action did not take place by chance methods; that obsessions and other neuroses develop according to some natural mode or modes; and that careful examination of enough cases will enable us to discover this pattern in each class of disorder. Hence the conclusion that all neurotic symptoms and dreams—which correspond to neurotic symptoms in 'normal' persons—are 'wish-fulfillments.' To quote Freud's words, a symptom "is fundamentally a wish-fulfillment, exactly as a dream" (**6**, p. 259); it is "a substitute satisfaction for that which is missed in life" (**6**, p. 260). Since this definition is irrelevant to our purpose, we may be spared the labor of tracing the tedious process by which Freud arrives at it (cf. **7**, pp. 1–79, 465 ff.).

But just as orthodox medicine had presupposed a theory of the workings of the body, so psychoanalysis required a theory of the 'inner workings of the mind.' In this way, psychoanalysis, the art, gave rise to psychoanalysis, the psychology.

In analyzing and criticizing Freud's psychology, the first thing to be said is that it does not constitute a complete and coherent system. Martin W. Peck correctly states the case when he says that "there is nowhere in the literature a systematic presentation of psychoanalytic psychology" (**15**). But regardless of the coherence of its several concepts, it was inevitable that a theory of normal psychological activity, however distorted in the direction of the abnormal, should crystallize out of the clinical results of a practice as extensive and successful as Freud's. This body of doctrine, scattered throughout his writings, Freud calls *metapsychology*. As I. Levine says, "metapsychology is a general name for the theory which underlies Freud's account of psychic life. It is the psychological foundation of psycho-analysis" (**12**, p. 114). This term, like *psychoanalysis*, was coined by Freud, and signifies a threefold interpretation of psychological phenomena. He proposes "that when a psychic process can be successfully described in its *dynamic*, *topographic*, *and eco-*

nomic relations, that should be called a metapsychological presentation" (cf. **12**, p. 114, and **8**). A brief resumé of metapsychological doctrine should, therefore, indicate the salient features of Freud's psychology.

Apart from the reception of external stimuli, all mental life may be regarded as an interplay of forces. These forces may conflict or compromise, inhibit or combine,—the various mental processes being the net result. Ontogenetically, these forces revert to natural instincts, and therefore have an organic origin. Instinct seems to be but vaguely differentiated from idea-with-an-affective-charge, the only difference being the rôle of the environment in the causation of the latter. But insofar as the environment may be eliminated from consideration—and the very inclusion of more than one way of regarding psychological activity illustrates Freud's confusion on this subject—the main concepts used are *dynamic*. The distinction between them tends to drop entirely out of sight when they are considered as 'inner excitations.' There are two groups of instincts: the ego-instincts, the purpose of which is self-preservation; and the object-instincts directed toward relations to external objects. Behind these instincts seem to lie Eros (the instinct for ever closer union) and the instinct for destruction (which leads toward the dissolution of what is living). In the face of the denunciations of the 'pan-sexuality' of psychoanalysis, special attention deserves to be called to this division, apart from which such concepts as the pleasure-principle and the reality-principle, ego and ideal-ego, fixation and regression, would lose much of their meaning (cf. **6**, p. 260). Theoretically, Freud has protected himself still further from such a criticism in declaring that the "significance of the sexual complexes must never . . . be exaggerated to the point of being considered exclusive" (**7**, pp. 240–241).

From the *topographical* point of view, psychological activity is regarded (Freud appears to be an excellent visualizer!) as an apparatus or instrument composed of three parts, the id, the ego, and the super-ego. The id is the 'reservoir' of instinctive impulses, the storehouse of dynamic ideas. As is

well-known, this is the same 'part' of the psychic anatomy, essentially, as the unconscious, except that possibly the latter concept focuses attention more directly on what is repressed rather than on what represses. The ego is the most 'superficial portion' of the id, that portion which alone is modified by stimuli from the external world. Consciousness is 'the ego's outermost layer,' the sole purpose of which is perception; it is a 'sensory organ' which 'perceives a content presented from another source.' These sources are twofold, corresponding to the two sides of the layer, the external world and the fore-conscious (7, pp. 121, 453). Freud's reason for restricting the meaning of consciousness rests upon what seems to him to be the irreconcilability of permanent registration and effective operation: ". . . obvious difficulties arise if one and the same system faithfully preserves changes in its elements and still remains fresh and capable of admitting new motives for change . . . we shall distribute these two activities among two different systems. We assume that a first system of the apparatus takes up the stimuli of perception, but retains nothing of them—*i.e.*, it has no memory; and that behind this there lies a second system which transforms the momentary excitement of the first into lasting traces" (7, pp. 426–427). Although this memory system or layer which registers the indelible 'memory traces' lies next to the consciousness-layer, it would hardly be exact to identify it with the fore-conscious; for, while the fore-conscious consists of the unrepressed memories immediately accessible to consciousness, it also involves an element of 'direction' which is opposite to that of the memory system. Unlike the id and the ego, the super-ego, which, as the prefix implies, is that part of the instrument which controls the ego, is a developmental product. On the one hand, it represents the deep-lying motives of mankind. On the other hand, since it develops in accordance with the reality-principle, it has respect for the external environment, and therefore represents the inhibitions characteristic of the ordinary man.

With the notion of development, which follows a mean course between the hedonic propensities of the organism and

the stern necessities of the outer world, we are introduced to the third element of metapsychology. From the *economic* point of view each idea is conceived as possessing definite quanta of energy or affective charges. We have, of course, no standard of measure for these quanta, but are justified in making the assumption because we learn empirically that an increase or decrease of excitation is related to affective intensity. On the basis of such empirical findings, Freud believes that it is safe to assume also that pain is somehow related to an increase, pleasure to a decrease, in excitation. Thus if quanta of energy be added to any experienced pleasure, the resulting intensification will in time produce pain. In the interest of avoiding pain, therefore, the accumulation of energy must be regulated, and this is the very purpose of the mental apparatus.

This brief capitulation of Freud's metapsychology suggests certain generalizations, which, although they considerably overlap, may be set down separately. Probably the most general criticism to be offered is that Freud's psychology seems to stand in the shadow of Bacon's idols. For example, the conceded fact of distortion has proved a serious menace to an experimental study of dreams. Freud ingenuously converts this obstacle into an advantage by holding that the distortion is purposeful (**7**, pp. 405 ff.). Therefore, since the psyche operates according to fixed laws (cf. **19**), which carefully control dream work, our primary concern has to do with the practical business of uncovering the distorting motive of the dream. To assume the fixity of these laws of operation from the beginning is, of course, to postulate the conclusion to be proved; but some such *petitio principii* evidently forms the transition in Freud's thinking from the fact of dream distortion to the concept of psychic determinism, which Flügel (**5**) mentions as one of the two basic concepts of psychoanalysis, the other being the unconscious. Or take the animistic language, characteristic of Freud, with which he describes the pathogenic process of repression. "There is a force in the mind which exercises the functions of a censorship, and which excludes from conscious-

ness and from any influence upon action all tendencies which displease it" (**8**). Or take the concept of traumatic fixation which, when questions of utility are put aside, is decidedly reminiscent of somatic pathology (cf. **19**). But without subjecting the whole gamut of Freudian concepts to examination from these several quarters, we may confine ourselves arbitrarily to a brief statement of what appears to be evidence of that most troublesome class of prejudices, the idols of the market-place.

Not a little of Freud's difficulty from this quarter is traceable to his fondness for analogy, concerning which he says, "I think it superfluous to apologize for the imperfections of this and all similar figures. . . . I believe that we may give free rein to our assumptions provided we at the same time preserve our cool judgment and do not take the scaffolding for the building" (**7**, p. 425). The figure in question is that of the 'psychic apparatus' having 'component parts,' spatially arranged, reference to which has already been made. While Freud goes on to say that this figure is a fiction, the essential outlines of the simile are ingrained in the topographical section of his metapsychology. Despite his good intentions, therefore, he seems to have mistaken the scaffolding for the building; or, more exactly, in default of revision, his crude nomenclature becomes part and parcel of his system.

Another analogy seems to be imbedded in Freud's thought. "We will compare the system of the unconscious," he says, "to a large ante-chamber, in which the psychic impulses rub elbows with one another, as separate beings. There opens out of this ante-chamber another, smaller room, a sort of parlor, which consciousness occupies. But on the threshold between the two rooms there stands a watchman; he passes on the individual psychic impulses, censors them and will not let them into the parlor if they do not meet with his approval" (**6**, p. 256). Again, Freud recognizes the unscientific character of his language: "I know [these conceptions] are crude—indeed, we even know that they are incorrect, and if we are not very mistaken we have a better substitute for them in readiness." But before leaving the subject, he says in the same

paragraph, "I should like to assure you that these crude assumptions go far in approximating the actual situation—the two rooms, the watchman on the threshold between the two, and consciousness at the end of the second room in the role of an onlooker" (6, p. 257). Unfortunately, it is just such dualistic and animistic features that trouble the critical reader of the passage. Presumably, Freud intends his metapsychology as the 'better substitute.' But if he means it to be read without reference to this or some other analogy, it scarcely makes sense.

Specific words likewise appear to exert a reflex effect upon Freud's thought. 'Mechanism' is an example. Having ruled out consideration of the physiological, he cannot mean by mechanisms of symptoms (6, p. 220), mechanisms of dream-work (7, p. 297), the mechanism of association (7, p. 465), and the like, the physiological correlates of function. The explanation seems to lie in a pre-scientific attitude of common sense, according to which every word stands for an entity. In no other way does it seem possible to account for the disjunctive status acquired in Freud's thinking by such pseudo-entities as 'mechanisms,' 'systems,' and 'complexes,' to mention a few notable examples.

Apparently, Freud's neglect of the physiological factor serves as the entering wedge for a cleavage of mental and physical, which terminates in what is little less than a frank metaphysical dualism. The transition is gradual; but if we follow the growth of such a concept as the unconscious, we are likely to be convinced that it actually takes place. In his lecture on "Manifest dream content and latent dream thought," Freud cautiously approaches the 'hidden' or 'inaccessible' element of the dream, and describes it as 'unconscious *for the time being*' (6, p. 90). This is his first definition of the unconscious, a descriptive definition no more objectionable than it is new. But when he comes to the lecture on "Resistance and suppression," Freud alters the definition: "I now go into the theoretical ideas which alone have shown themselves useful in making the conception of repression more definite. It is above all necessary that we progress from a purely descrip-

tive meaning of the word 'unconscious' to its more systematic meaning . . . let us assume that every psychological process —with one exception, which I will go into later—first exists in an unconscious state or phase and only goes over from this into a conscious phase, much as a photographic picture is first a negative and then becomes a picture by being printed" (**6**, p. 255). Instead of being a limiting concept of the conscious, the unconscious is here a realm of potentialities, the 'reservoir' of mental forces which surge forward only to be repressed by the more powerful censor. Now, as Münsterberg once remarked, "if we call [the subconscious] a reservoir of ideas we have yielded the whole point; ideas are of mental stuff" (**14**, p. 21).

'Ideas' seem to slip into Freud's system as substantive entities while his attention is turned critically toward the unconscious. In the paragraph following his original definition of this concept, we are told that the task of dream interpretation is to discover 'this unconscious thing' for which the dream as a whole is a distorted substitute. As we proceed, we learn that this 'something unconscious' is the latent content, and this content consists of ideas, which, though repressed, are indestructible (**7**, p. 456). Since they possess energy of their own, these indestructible ideas, which one expositor of Freud carelessly describes as 'the germs of the nervous sick-processes' (**20**), force their way out of the unconscious even though they may find it necessary to take the form of a symptom in order to get past the censor. Thus while Freud is guarding the doorway, psychic substance enters at the back; and we are accordingly not surprised when he remarks, "you will understand then that in psychoanalysis we cannot do without this unconscious psyche, and are accustomed to deal with it as with something tangible" (**6**, p. 241).

Memory is, within limits, an exception to the assumption that every psychological process exists first in an unconscious state. As we have learned, the one aspect of psychological activity that cannot be subsumed under the dynamic point of view is the reception of stimuli. "The perceptions that come to us leave a trace in our psychic apparatus which we may call

a 'memory trace.' The function which relates to this memory trace we call the memory" (7, p. 426). Memory as a mode of operation, therefore, reverts for its explanation to a 'memory trace' impressed upon it by a causally efficient environment. Our interest here is not whether this explanation is correct, but to notice that the language and the context unmistakably denote a psychic substance akin to Locke's *tabula rasa*. Considerations such as these lead the critical reader to suspect that casual reference to a 'mysterious leap from the psychic to the physical' (6, p. 222) suggests a more serious problem than Freud is wont to believe.

Freud's motive in the beginning was practical, as we have seen, and there is nowhere clear evidence that it changes upon his entrance into psychological discussions. Had critics, as well as popular readers, remembered this fact, some of Freud's leading concepts would possibly have been received less belligerently. 'Sexuality' may serve as an example. Just as the attorney seeks to prove to the jury that concealed weapons, agitated actions, and report of the pistol constitute a direct line of evidence pointing back to an 'intent to kill,' so Freud endeavors to show that all loves, from the lowest to the highest, form an unbroken series which points back to sex as the sustaining principle. Psychologists object to such constructions on the same basis as that which led psychoanalysis to reject the biological interpretation of the symptom in terms of survival value. But so long as Freud does not foist such concepts bodily upon psychology, he is to be condemned no more than the attorney.

But here lies the crux of the problem, for Freud does not scruple at identifying the goal of psychoanalysis the psychology with that of psychoanalysis the art, even though he does partially differentiate the one from the other. It follows that whereas the science of psychology aims to *describe* its subject matter, Freud aims to *explain* his subject matter. The difference between his two definitions of the unconscious, for example, is to be accounted for in this way. In consequence, Freud finds himself confronted with the philosophical problem of causation, his treatment of which comes into special prom-

inence in connection with the dynamic aspect of his system. This feature is described by Levine in emphatic, if prolix, language as 'the very central essence of Freud's psychology' (**12**, p. 115). The dynamic aspect of psychoanalysis has been generally linked with voluntarism, thus receiving its justification by many Freudians on metaphysical grounds. Freud himself has little to say by way of justification, believing the problem to be one which psychoanalysis is incapable of solving (**6**, p. 357). But strangely enough—possibly because he saw that the spontaneity of his system tended to violate the principle of conservation; possibly because of his belief that psychoanalysis and psychiatry (which usually accepts the point of view of biology) supplement each other—what he has said seems to controvert voluntarism or vitalism in any form. All dynamic activities, of course, revert finally to the original instincts. But what is an instinct? Though admittedly an 'obscure' subject, Freud tells us that an instinct is "the conservative nature of living beings," "a tendency innate in the living organism which leads to the repetition of a former condition" (**12**, p. 138). Presenting some conclusions which he believes 'accord closely with those Freud has reached,' Ernest Jones says, with reference to inhibiting forces and the like, ". . . one must imagine that the passing millenia leave their imprint" (**11**). "In the last resort," therefore, to quote another sympathetic expositor, instinct "is no doubt the product of external forces which have left their imprints in the organism" (**12**, p. 137). Moreover, this is substantially the same position as that taken by Freud with reference to the phantasy, which he holds to be a 'phylogenetic possession' (**6**, p. 323).

As further evidence of the non-dynamic elements in Freud, we may turn to the psychological activities as such. The psychic apparatus, to which we have referred, is a 'reflex apparatus.' "The reflex act remains the model for every psychic activity" (**7**, p. 426). In accordance with this schema, "stimuli act upon the psyche, and it must react to them" (**6**, pp. 68, 70). Character is based upon 'the memory traces of our impressions.' Every dream has a stimulus (**7**,

p. 139); the dream is 'under compulsion' to elaborate all the dream stimuli (7, pp. 192, 151); in fact, all 'inner excitations' are referred to occasionally as 'stimulus-masses.'

From what has been said, it appears that Freud's psychology is a confused medley of dynamic and mechanical elements. It is not our purpose to determine as to which of his views is correct, or whether they are mutually exclusive. But the fact that they stand side by side, neither mechanism nor vitalism being portrayed consistently, betrays a carelessness in handling the principle of causality which the history of psychology condemns as inexcusable.

Therefore, if we came out to see a 'new' system, we shall be disappointed in what Freud has to offer. We miss in him the relentless logic of the system-builder. But this need not detract from the real service he has rendered psychology, a service that in many respects resembles that of Darwin to biology. Both were pioneers; both collected a wealth of valuable material; and both offered stimulating interpretations of their respective bodies of data. But just as the hypotheses of the one awaited revision at the hands of men like Mendel and De Vries, so the hypotheses of the other await revision at the hands of the experimental psychologist.

If this lends to Freudian psychology a tentativeness utterly foreign to the dogmatism of many Freudians, it does little violence indeed to the attitude of Freud himself. Thus he writes, "Psychoanalysis is founded securely upon the observation of the facts of mental life; and for that very reason its theoretical superstructure is still incomplete and subject to constant alteration " (8). In another place he says even more emphatically, "Let anyone in the world account for these facts in a more correct scientific manner, and we will gladly withdraw completely our assumption of unconscious psychological processes" (6, p. 240).

When all the threads of Freud's psychology are drawn together, probably the most important lesson he has to teach is that man, at any moment of his life from birth to death, is the product of his past. As Peck observes, "Stripped to its bare bones and given in merest outline, Freudian psychology,

normal and abnormal, is explained by the libido development and its vicissitudes" (15). J. W. Bridges offers a similar summary statement: "The fundamental contentions of psychoanalysis may, in the writer's opinion, be summed up in a single paragraph. Man is a product of inheritance and environment, of nature and nurture" (2).

Psychoanalytic study of psychological activities in their genetic setting, revealing the role of 'unconscious' psychological activities, appears to have thrust upon psychologists the necessity of revamping the traditional view of the limits of their subject matter. Psychology has been thought of—though an ever-growing body of experimental material fits the view awkwardly—as the science of consciousness, or of conscious activities. As one writer states the position, "The mental and the conscious are coextensive terms" (3). There is even reason to believe that Freud's notion of an 'unconscious,' paradoxically enough, embodies at heart such a view. For such a circular direction could hardly have been followed by the controversy over the 'unconscious' had not the opponents been arguing from the same, or at least similar, premises. The two sides seem to be agreed for the most part (Freud vacillates on his side, possibly for the reason given) on what seems after all to be the important matter, namely that psychological activity must be homogeneous and therefore describable in a common language, regardless of the category of description appropriated. Confusion arises over the question of the common term. His opponents would explain the unconscious in terms of the conscious, while Freud, reversing the procedure once he has made the transition from the descriptive to the explanatory unconscious, all but explains away the conscious in terms of the unconscious. Fortunately, the body of empirical material which he has collected is not so much at cross-purposes with itself. This material furnishes more or less conclusive evidence that the 'conscious' does not comprehend all psychological activity; but it is still psychological activity, nevertheless, and does not need to be chopped up into heterogeneous elements with doubtful labels. The continuity here suggested may hardly be envisaged so long as

we use such correlatives as 'conscious' and 'unconscious.' Because of the confusion associated with the one and inherent in the other, it appears that these terms have served their day and deserve to be laid to rest, with due respect.

Freud's embarrassment in dealing with the category of substance, on the one hand, and the contrast between the substantive and functional language involved in the discussion up to this point, on the other, raises the interesting question as to whether the facts of psychoanalysis may be legitimately and consistently described in terms of function. With regard to this question two things are fairly obvious. First, if by 'function' is meant what the speculative biologist means, namely the teleological notion of adaptation, then such an undertaking is doomed at the beginning. This should be clear from what has been said. Secondly, if by 'function' a descriptive instead of an explanatory goal is implied, Freud's psychology *qua* body of doctrine must undergo serious modification. This, too, should be clear from what has been said with reference both to the goal of Freud's psychology and the futility of transliteration from one category to another. But it is unnecessary to identify 'function' with the purposive act of the biologist. And it is illogical to confuse the practical results of psychoanalysis with Freud's explanation of them.

Function signifies either a mode of operation or a kind of accomplishment (cf. 1, p. 200). The two uses of the term may be illustrated by quoting from one of the most popular current histories of psychology: "The accepted functionalism rests primarily upon biology. The *function* that interested its early adherents was the capacity of the animal to adapt itself in the environment, and mind was the sum total of the capacities that made such adaptation possible. One could perfectly well apply the term to mean a study of the *functions* of consciousness apart from the mental states that conceivably make *these accomplishments* possible" (16, p. 277, italics mine). In the first sentence, 'function' seems to mean a kind of accomplishment; in the second, a mode of operation. It is in this second sense (the category in terms of which the author of the quotation believes that the subject matter of

psychology may be written) that function is opposed to substance.

Adequate development of the thesis that the empirical results *per se* of psychoanalysis may be described in terms of the category of function would carry us considerably beyond the limits of the present article.[2] But one who turns to Freudian literature with the problem in mind can hardly fail to be impressed by the fact that such a functionalism is implicit in these writings. It is overlaid with ponderous theoretical concepts, but it is there nevertheless. A few quotations may be cited by way of illustration. The italics in each instance are mine.

Psychoanalysis "seeks to ascertain the inner *workings* of the human soul . . ." (**6**, p. 220). Elsewhere, Freud speaks of the "construction and *workings* of the psychic instrument" (**7**, p. 405). Stanley Hall, in his preface to Freud's *General Introduction*, expresses the opinion that "the psychoanalysts . . . have been somewhat too ready to apply their findings to the *operations* of the normal mind" (**6**, p. vii). Discussing his 'economic' principle, Freud says, "We ask ourselves whether a fundamental purpose is recognizable in the *workings* of our psychological apparatus, and answer immediately that this purpose is the pursuit of pleasurable excitement" (**6**, p. 308). In a similar context Levine comments, "The excitations from within the organism, which are also perceived by consciousness, have affected its *function* more [than the impinging of stimuli from without]. For it is the task of dealing with these inner excitations which has really given to the apparatus its distinctive *mode of operation*" (**12**, p. 136).

Freud describes the 'instinctive tendencies' as '*functions*' (**6**, p. 376), and these tendencies lie at the end of 'the causal chain far beyond the suppressions.' Introducing his lecture on etiology, he says, "I believe we are in harmony with the teachings of general pathology in assuming that this development involves two dangers, inhibition and regression. In other words, with the universal tendency of biological processes toward variation, it must necessarily happen that

[2] The reader is referred to Bentley's chapter in Psychologies of 1930, pp. 95–114.

not all preparatory phases of a given *function* are equally well passed through. . . . Certain components of a *function* may be permanently held back in an early stage of development . . ." (6, p. 294). With reference to the other danger, inhibition, Freud inquires later, "In what way shall we now account for the observation that the patient so energetically resists our attempts to rid him of his symptoms and to make his psychic processes *function* in a normal way?" (6, p. 254). The question obviously implies that the symptom is a functional product, and this implication is confirmed by such utterances as, "Now we know where the forces whose existence we suspect must *operate*" (6, p. 254), although the suggestion of a place 'where' has decidedly different implications.

The clinical problem, Freud informs us, is "to discover *how* persons become sick, *how* they later on *accomplish* a neurotic adaptation toward life" (6, p. 244). When he speaks elsewhere of the psychoanalytic "insight into the *mechanism* of these symptoms" (6, p. 220), he colors this problem with a figure borrowed from somatic pathology (cf. 6, p. 376). Stripped of this figurative character, a 'mechanism' seems to be nothing more than a pen-picture, or sketch, of the *type of operation-course* that terminates genetically in a 'symptom.' And this meaning crops out, despite substantive and causal coloring, as when Freud remarks that under the influence of the unconscious, a jest "experiences the *workings of the mechanisms* there in force, namely, of condensation and displacement; that is, of the same processes which we found active in the dream work, and it is to this agreement that we are to ascribe the similarity between wit and the dream, wherever it occurs" (6, p. 201). The italicized phrase is equivalent to the tautology, 'the workings of the workings.'

In consideration of the importance that Freud attaches to the dream, possibly the most significant statement with reference to the problem at hand occurs in a paragraph in which the author summarizes a lengthy discussion on dream-work. "The psychic activity in dream formation," he says, "resolves itself into *two functions*—the provision of the dream

thoughts and the transformation of these into the dream content" (7, p. 401). "The dream, therefore," he repeats in another work, "is the *way in which* the psyche reacts to the stimuli acting upon it in the sleeping condition" (6, p. 68). "Reviewing . . . as a whole the process of dream-making," Ernest Jones, in an early article, wishes "to lay stress on the fact that in the formation of a dream no intellectual *operation* of any sort is carried out; the dream-making is concerned solely with translating into another form various underlying dream thoughts that were previously in existence" (10). The accuracy of Jones's interpretation is questioned by the following statement by Freud, but the discrepancy does not pertain to the point of immediate concern to us: "The psychic *function* in dream formation . . . aspires to the original creations only in the most extreme cases; whenever possible, it makes use of anything available it can find in the dream material" (7, p. 391). The process by which the dream material is selected is an '*operation*'.

After contrasting the unconscious and the fore-conscious, Freud observes, "In our eyes the characteristics of each of the two systems were betrayed by this *difference in their functioning*" (6, p. 258). Similarly, as one of his expositors tells us, "The lowest level of the unconscious is thus far removed from consciousness in its *modes of functioning*" (3). The difference is due to the phenomenon of repression, according to which a shock consciously beyond recall may yet be 'existing and *working*' (3). 'Instances of the *same mysterious workings*' may be found in the sudden solution of a knotty problem, or in the sudden recall of a forgotten name (3). In connection with the same general problem, H. K. Haeberlin explains, "It was found that the temporal succession of many ideas, emotions, or volitions could not be converted directly into a causal relation without assuming the *operation* of certain intermediary psychic processes" (9). Similarly, Adrian Stephen, a critic of psychoanalysis, says by way of definition, "A repression is an emotion, thought, wish, anxiety, or the like, which is kept out of consciousness by the *operation*, conscious or unconscious, of some shame,

pain, or aversion with which it is for some reason associated" (18).

From these random quotations it appears that after all psychoanalysis is primarily concerned with functions. If it is true, as it seems to be, that the reason for the neglect of these functions as such lies in the undue prominence of certain philosophical concepts, perhaps psychoanalysis would profit by a revision of its theoretical basis, beginning with an equitable division of labor between philosophy and psychology.

By way of summary, attention may be called to the emphasis laid on the distinction, which Freud himself recognizes, between psychoanalysis the art and psychoanalysis the psychology. The former arose as a method of treating nervous patients medically; the latter, as a doctrinal basis for this method. The problem, therefore, for which Freud's psychology, which comprises this body of doctrine, offers a solution is the interpretation of clinical material which exhibits a decided obtuseness to the objective categories of physiology and biology.

Although it is thus to a practical need that psychoanalysis owes its origin, it is largely the practical motive that vitiates Freud's psychology as a system. His uncritical formulation of many of his concepts leaves him vulnerable to the charge of consorting with Bacon's idols. No causal link has been inserted between this pre-scientific attitude and the fact that Freud substitutes psychic substance for somatic substance and proceeds thereafter, in his treatment of 'mental disease,' in a fashion analogous to that of his medical colleague in somatic pathology; but his handling of the concept of substance is, to say the least, doubtful on the principle of parsimony. Likewise, Freud's unsatisfactory treatment of the problem of causation, arising also from his practical interest, has been set down as a vitiating element of his system; and the negative importance for scientific purposes of this concept thereby disclosed suggests that for psychological purposes causal etiology may be supplanted by descriptive etiology.

Probably the greatest contribution made thus far by

psychoanalysis to psychology has resulted from the study of psychological activity in the light of its genetic setting. In this way psychoanalysis, corroborating experimental evidence, has drawn attention to 'unconscious' factors which—although historically they have tended to distort psychology in the direction of the abnormal—call for a formal revision of the limits of psychological subject matter.

Since psychoanalysis violates the division of labor between philosophy and psychology in offering an explanation of its subject matter, the question has been raised in conclusion as to whether this subject matter lends itself to description in terms of function. When this category is defined as a mode of operating, as a way of working, we are apparently justified in describing the subject matter of psychoanalysis as consisting of functions and functional products.

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THE WILL-O'-THE-WISP "INTELLIGENCE"

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An examination of the index of the *Psychological Abstracts*, published for December, 1930, reveals that the term 'Intelligence' ranks fourth in the number of citations and is surpassed only by citations under the headings 'Child,' 'Education,' and 'Tests.' Without considering the tremendous overlapping which is implicit in these four headings, it is obvious that 'Intelligence' is of great importance in the psychological literature of today. It is likewise obvious, from the content of many of the cited articles, that there is a wide diversity of opinion concerning the nature of this very important phenomenon. As a corollary to the various definitions of intelligence we have dozens of tests to measure this elusive quality, many of the tests differing from many others according to the aims and definitions of the test-makers, and, as Johnson (3) points out, all the tests purporting to measure indirectly something which can neither be measured nor demonstrated directly. From all the welter of confusion on this topic, one has cause to wonder if 'intelligence' and 'intelligent behavior' have, as such, any existence at all.

The various definitions of intelligence seem to boil down to the general meaning: the ability of (or the capacity for) adapting adequately to the environment, an adequate and efficient adaptation being either concrete or abstract in nature. Now this sort of definition really means nothing at all, for we have no criterion for adequacy of adaptation, or, perhaps, we have too many different criteria, which amounts to the same thing. Was the adaptation of Columbus adequate, or not? And is his adaptation to be judged on the basis of opinion prevalent just before he sailed, or on the basis of opinion prevalent sometime after he sailed? Were any of the martyrs intelligent who suffered the definite and horrible biological maladap-

tation of death at the stake? Or, to bring the matter closer home, did Woodrow Wilson made an adequate adaptation at the Peace Conference? Thousands of people think that he did, and worship him—thousands think that he failed utterly. Perhaps the best criterion of adequacy of adaptation, for you, is what you think is a good thing to do, and, for me, what I think is a good thing to do—which is the same as saying that perhaps we have too many criteria of adequacy of adaptation.

Edwards (2) would avoid this necessity for evaluating behavior by saying, "It is proposed to define intelligence as *capacity for variability of response*," which is non-speculative but which renders useless the word to be defined, since it would be simpler to say that individuals vary in their variabilities of response, omitting entirely the word 'intelligence.' Peterson (7) avoids the pitfalls of evaluation, and, very definitely, the pitfalls of a non-scientific animism: "Now intelligence, in the view here taken, is not a force at all: it is not a power that effects adjustment, but is only a mechanism through which or by means of which the adjustment is brought about. For our purposes we shall define intelligence as a biological mechanism by which diverse impulses are brought together and given a unified and somewhat consistent effect in behavior. These impulses are, of course, the results of stimuli of various kinds—interoceptive, proprioceptive, and exteroceptive—both simultaneous and successive, direct and indirect." While there may be little objection to this, it may be pointed out that the body as a whole must be the unifying mechanism and that the total structure (or function) of the body is the total intelligence of the embodied individual. This will be stressed a little later. The point at present is that if we assume that there is intelligence, we have considerable difficulty in pinning it down to any one place, or activity, or criterion.

Suppose that we do assume that there is intelligence, and that certain adaptations are pragmatically better than others. Then we might speak of differences in the adaptations of organisms, but not of differences in their capacities for intelligence. An adequate adaptation can be shown only in adapt-

ing. If Rodin s 'Thinker' removed his chin from his hand only to say that periods of industrial depression are usually followed by periods of prosperity, we would hardly venerate his posture as indicating intelligence. Again, the adaptation of the greatest intellectual giant varies not an iota from the adaptation of the most stupid idiot, when both of them are in a state of coma. This idea that intelligence is shown only in acting is indicated by many writers when they prefer the term 'intelligent behavior' to the term 'intelligence.' There seems to be some evidence that when certain organisms do get around to acting, their behavior will be more adequate than the behavior of other organisms, in which case we may speak of an organism's likelihood of adapting. The term 'capacity' is open to certain objections when used in the sense of 'likelihood'—'capacity' implies an intangible power resident in the individual. A capacity can be known objectively and accurately only when it becomes manifest, in which case it is an actuality rather than a capacity. As an illustration of likelihood of adapting, we may consider the case of the baseball player. His adequacy along certain lines may be rather objectively measured, so that he has a batting average and a fielding average. These averages indicate the actuality of his past performance, and suggest what he is likely to do at such future times when he is performing. On the other hand, the player's 'capacity' for playing errorless ball and for getting base hits may vary from day to day, and may not be known absolutely.

There is, however, an objection to the term 'intelligent behavior,' as indicating a distinct species of behavior. All behavior, all activity, can be thought of as motivated behavior, and the living organism may be thought of as a dynamic organism. The Freudians may speak of the motivating force as the libido, and the Behavioristis may speak of it more definitely as stomach contractions, disequilibrium of the sex organs, and so on. In any case, we are assured that there is a cause, motive, or what-have-you, for every reaction. From this standpoint, all activity is purposive activity if there be a stimulus for it. This dynamic view of behavior, then, is that

every activity has some motivating force behind it, from the primary drives of tissue disequilibrium to the most highly modified or most subtle activity of the 'reasoning' sort. The behavior is primarily trial-and-error behavior, falling into habit patterns with repetition, and is continuous due to the presence of a continuing stimulation. If the behavior be a response to a stimulus, it must in some degree be adaptive behavior. Thus it follows that we cannot speak of intelligent behavior as qualitatively different from any other sort of behavior, whether it be emotional, or habitual, or reasoning, or anything else. For any given individual there is a 'stream of behavior' influenced by the complexities of the organism, the past experience, and the present environment, and this behavior, being adaptive behavior, is always intelligent to some degree—always assuming that there is intelligence and that certain adaptations are pragmatically better than others.

With regard to emotional and intelligent behavior, the present writer agrees heartily with Woolbert (9): "We can suggest an understandable relation between 'the intellectual' and 'the emotional' by saying that what we call intellectual processes are those which tend to intensify tonicity and movement in a narrow and restricted field, while what we call emotional processes tend to raise or lower tonicity over an area approximating totality. . . . Inasmuch as all of us start life totally emotional, with almost no powers of discrimination, our education, as is commonly agreed, is a process of gaining powers of discrimination and selection. Behavioristically, this is a process of reducing the spread of all local tensions; though practically all local intensities extend, at lower degrees of intensity, to the whole organism." (It is interesting to note here an implication of physiological gradients, and of Coghill's thesis (1) that isolated movements are 'carved out' of total behavior patterns.) If there be only a quantitative difference between 'intellectual' and 'emotional' behavior, it would seem somewhat difficult to classify the behavior definitely, and with any pretensions to logic, under our conventional rubrics. To say that behavior is intelligent, or emotional, or habitual, or instinctive, and so on, would

seem to be like saying that an orange is round, or yellow, or sweet, or heavy. The orange is all of these things at once, if it is anything at all, and behavior is of all sorts at one time, if it is any sort at all. To state the same thing in another way, when the organism acts it acts as a unit and not by faculties, and every reaction may influence or be influenced by the acting of every muscle cell, nerve fiber, and sensory structure in the whole organism. Lashley's work (**4**) may be cited to indicate the interdependence, rather than the independence, of the parts of the living organism.

Thus, if we assume that there is 'intelligence' or 'intelligent behavior,' we must imply a distinction between this and 'non-intelligence' or 'non-intelligent behavior.' This distinction would be extremely hard to make. Truly, this 'intelligence' is an elusive thing, hiding now in one part or in one activity of the organism, and now in another part or in another activity.

The present writer is concerned with two other, related problems: first, the customary classification of abnormalities into qualitative forms (as psychoses) and quantitative forms (as feeblemindedness); and, second, the uses of 'intelligence tests.'

Dr. Florence Mateer (**5**), in her valuable book *The Unstable Child*, points out the difference between quantitative and qualitative abnormalities: ". . . The mental-age test has given us a rating of his 'level.' The psychopathy of an individual is a statement of the extent to which that intelligence with which he is equipped works normally or abnormally. . . ." And, "To reiterate: The study of intelligence level is a study of our intellectual resources, our capital, or our raw material. The study of function is a study of the efficiency of the utilization of these resources. Mental-age ratings give us the presence of, or lack of, intelligence. Functional analyses give us the presence of, or absence of, disturbances of intelligence. Our final psychological estimate of an individual's ability is a summing up of how well he uses the mental equipment he has. . . ." And, "Another complexity lies in the fact that the presence of a definite psychopathic tendency is apt

to evidence itself in an actual lowering of the intelligence rating which a child can achieve." Without wishing to minimize Dr. Mateer's outstanding contributions, the present writer would interpret these quotations to mean that each individual has a certain 'intelligence' which he may or may not use at a certain time. If he acts in a peculiar fashion, he may lower his intelligence rating—but it is hard to seen what his rating is unless it is a statement of how he acts. If a man six feet tall were to stoop until the top of his head is only five feet from the ground, then, at that moment, the top of his head *is* only five feet above the ground. He may later stand on a chair, so that the top of his head is eight feet from the ground, and at that time the top of his head *is* eight feet from the ground. It is admitted that the analogy breaks down when the objection is made that, under standard conditions of measurement, the man is actually six feet tall—but the question may be asked, 'What are the standard conditions of measuring intelligence?' and the present writer is ignorant of the answer. If we prescribe how a subject shall act under test conditions, then we prescribe to a large extent what score he shall make. And if we refuse to test a subject who does not behave exactly as most other persons behave, we beg the question with regard to that particular subject's intelligence. The point here is that the adequacy of an individual's behavior (or his conforming to the demands of environment or social custom) should be considered with respect to *all* of his behavior rather than with respect to his behavior on intelligence tests alone. The homicidal maniac may have a high I.Q., yet, in the last analysis, his behavior may be less adaptive than that of the stolid imbecile. Rather than classifying individuals into convenient pigeonholes, it might be more nearly correct to say 'with respect to a given type of activity (*e.g.*, refraining from murdering people; learning routine tasks; inventing electric lights; theorizing about intelligence; believing that angels whisper into one's ear) a given individual does not conform to social custom or to the demands of the environment as do most individuals,' or, 'this particular behavior is typical of only 6 per cent of the population.'

At present it may be convenient to classify people from the standpoints of intelligence and psychopathy, but the present writer would submit that this double classification may do great harm to psychology. If psychology is to claim standing as a science (which somehow seems quite fashionable and desirable at the present moment) it would seem necessary to hold fast to two concepts: that of determinism in behavior, and that of the reducibility of its data to quantitative rather than qualitative differences. Now, when the psychopathic patient is asked, in the course of an intelligence test, to mention three differences between a president and a king and he says that he would like to be a king, we have no more warrant for estimating his intelligence as high but disturbed than we have for so estimating the intelligence of the moron who suggests that a king is English and a president American. In both cases, from a deterministic point of view, each subject is behaving in the only way that he can behave at that time, and each one is failing to conform to the usual type of behavior. It is rather difficult to see why one of them is qualitatively abnormal, while the other is quantitatively subnormal.

And now to, round out the circle, perhaps, the present writer confesses to a belief in the values of 'intelligence tests,' if the tests be viewed from a certain angle. The position lately taken by Thorndike (**8**) seems to be an extremely sound one: "The great merit of the Binet Test is that it is a graded scale for intellectual difficulty, and it is only weakened by being interpreted loosely as a measure of some mysterious essence called intelligence which grows in man." It used to be a source of annoyance to the present writer when his students spoke of tests as 'I.Q. tests,' and the students were gently corrected: "You mean 'intelligence tests,'" but perhaps the students were right after all. The tests *do* deliver an I.Q. certainly. And that I.Q. does have some value, depending of course upon the test. If it has been shown that subjects earning high I.Q.'s on a certain test also achieve excellant grades in elementary school work, then that test is a reputable means of predicting school success or failure; if it be shown that individuals earning high I.Q.'s almost always

made financial successes, then the test would be an excellant means of predicting future incomes. Evidence for the first proposition is plentiful; it is sadly incomplete for the second. Intelligence tests, as has been previously suggested (**6**), may be considered as samples of behavior and the results worth no more and no less than the results of any other sampling process of comparable precision. By studying the previous behavior of a race horse before laying one's bets, one is less sure to lose one's money than if one bets consistently according to the color of the horse or the euphony of its name. In the same manner, by studying the past performance of a human on a particular set of problems, one may frequently guess what that human's future performance on similar problems may approximate.

If, however, we look upon intelligence tests as measurements of a specific entity, we fall into a very common error —the error of taking an 'I.Q.' or an 'M.A.' as the person's 'intelligence,' which seems to be at the basis of the tendency to divide people into groups of 'low intelligence' and of 'behavior abnormalities.' Now, if a ten-year old-boy 'acts like a six-year-old' (*i.e.*, has a mental age of six years) when confronted with such problems as counting pennies, showing his right hand, and so on, it is quite likely that he will act like a six-year-old on some similar problems, but it is also quite likely that he will not act like a six-year-old on certain other problems. It has happened that a subject will act like a six-year-old when confronted with the problems of the Stanford-Binet; like a seven-year-old when confronted with the problems of the Pintner-Paterson performance scale; like a five-year-old when confronted with the problems of the Herring-Binet. Now, if we view the M.A. and I.Q. as inspired and infallible statements of 'intelligence,' we are put in a very embarrassing position, unless we select one test as being the true test, and the others as impostors. However, if we view every intelligence test as an achievement test of some sort, and stop bothering about 'intelligence,' we may be more comfortable and somewhat more logical. It is certainly true that every test depends upon learned reactions, and even if

the opportunity for learning certain reactions is tolerably universal these learned reactions *are* achievements. They *may be* indications of native ability. Our main difficulty seems to be that the tests were devised by educators, and we educators are prone to give tests and examinations in class-rooms and out of them, and grade students on the more or less gratuitous assumption that we are measuring what we want to measure.

In short, the present writer would suggest that the I.Q. may be considered as a 'batting average' for certain types of adaptations when it has been shown that there is a definitely high relationship between those adaptations and the I.Q., and only for those adaptations. We do not take Babe Ruth's actual batting average to indicate his likelihood of writing good detective stories, but only to indicate his likelihood of getting base hits over a period of time. The objections to 'intelligence tests' is not to the tests themselves, but to the customary broad and general interpretation of results which actually are based on certain rather specific performances.

It would seem advisable to relegate 'intelligence' to the limbo whither 'instinct' is being relegated, and, having freed ourselves from the embarrassing obstruction of a name, to consider more critically the behavior of the individual *as* behavior.

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AN ANALYSIS OF MOTIVATION

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In his cautious moments, the psychologist will recognize but one essential antecedent of a response—the discharge of the axon of an efferent neuron, which in turn must have been preceded by the discharge of a neuron leading from a receptor. Thus, while there is no point-to-point relationship, either spatial or temporal, between stimulus-pattern and response-pattern, it is nevertheless granted that a succession of responses requires a sucecssion of stimulations, and that these stimulations must be fairly *immediate* antecedents. We have no basis for believing that at some point beyond a receptor—either in afferent or efferent neurons or in muscle itself—a response may be initiated spontaneously, or that a successive discharge may proceed independently from such a point, or that any impulse may be retained indefinitely before it is finally released.

Tradition, and the support of competent opinion, still lend weight to a single divergent hypothesis: The higher centers are held capable of *continuing* a neural process for some time by an interchange of impulses among themselves. To take one instance, Dunlap (**6**) thinks it possible that an interchange between cerebrum and cerebellum may, under proper conditions, be sufficient to maintain a thought-series. However, most exponents of the response theory of consciousness would maintain that kinesthetic patterns are necessary links in every train of associations; that the effector discharge constituting or paralleling a thought can only evoke a new thought-response by setting up a new kinesthetic stimulus-pattern.

Several other hypotheses are current which involve the special ability of higher centers to continue impulses independently; for example, 'long-trace' conditioned reflexes are

explained in this manner. (12 p. 88 f.) Granting the possibility that such assumptions may eventually be substantiated, there is no evidence at present which should cause us to rule out other, simpler hypotheses.

We start, then, with our systematic and natural picture of stimuli impinging upon receptors; of neurons discharging along pathways which have been determined by learning experiences; of innervated muscles exploding stored energy in patterns corresponding to efferent neural patterns—all of these sequences being necessary to every response, and occurring in immediate succession. On entering the field of motivation, however, we find a host of additional concepts, many of them quite incompatible with any simple physiological account of behavior. For one thing, we find terms for *enduring entities* which are held to determine behavior, such as Motives, Attitudes, and Character Traits.

In addition, several common terms enjoy a variety of meanings, some of which are acceptable for technical use, while others are more picturesque than precise. Concepts of genuine value become sources of confusion through loose usage.

The list of terms and definitions given at the end of this article is intended ultimately to clarify our conception of motives and of motivation; but as it stands, it presents an interesting picture of present incoherence. A number of the more recent texts have made great strides toward systematic perfection. As a rule, however, we find that one field at least —that of human social behavior—has remained quite proof against simplification; while even animal study, with its ambiguous 'drives,' shows a slight tendency toward confusion.

The Stimulus-Response Sequence.—Although we are striving for simplicity of terminology, it must nevertheless be noted that any receptor-to-effector sequence presents a highly complicated picture in nature. Any single reaction is the result of a multitude of stimuli, while at the same time any single stimulus-detail may influence many or all of the effectors in the body. It follows that the learning process cannot be as simple as the conditioned-reflex formula would

make it out. It is not necessary to elaborate here upon the qualitative principles observable in the conditioning process. We shall merely mention those which are relevant to later discussion. These include the Principle of Redintegration; the Law of Effect (which can be variously expressed); the Principle of the Algebraic Summation of Stimuli (which refers to the fact that stimulus-elements in a situation are of varying potencies, and may reinforce or counteract each other in the production of a response).

By the potency of a stimulus is meant (*a*) its capacity for arousing overt behavior, or its likelihood of being dominant—of arousing its peculiar response in competition with incompatible stimuli. Potency also refers to (*b*) its capacity for bringing about unpleasantness, or annoyance, when the responses which it evokes do not eliminate it readily.

Unpleasantness, characteristic element of subjective states, is difficult to interpret in physiological terms. We may observe, however, that since both feeling-state and activity depend upon the potency of stimulation, unpleasantness is correlated highly with tension and with restlessness of behavior.

Conflict and Choice.—The several parts of a situation may evoke incompatible performances, and may be so related in dominance that none of them can cause its own response to be carried out overtly; *i.e.*, none of them may be able to dominate over the others. In such cases we have the phenomenon of indecision or of conflict, identified objectively by strained posture, wavering in tentative responses, etc. Characteristic mental events occur at the same time.

To the situation described, a series of implicit responses (ideas of immediate response or of end-situation) occur in turn, and that one is finally carried out which becomes dominant as a result of this process.[1] Subjectively, this process

[1] Why the occurrence of a thought should make a response more probable of arousal by a certain situation, is of course the most interesting problem in connection with conflict and purpose. We can say with considerable safety that the occurrence of a thought, under proper conditions, modifies the nervous system—forms a trace, as in learning, which connects certain stimuli to certain effectors. But further enlightenment as to the mechanics of this process is not readily forthcoming. Present data yield only the vague suggestion that the feeling-state evoked by the experience of a purpose or impulse may be significant.

is experienced as *planning*, or *deliberation*, and the organic state of relief following the final, accepted idea of action is experienced as *decision* or *approval*.

When the stimuli in such an event are highly potent and are long continued without effective response, neither becoming dominant over the other, the state of *conflict* is experienced. There occur a succession of incompatible ideas of action (implicit responses) accompanied by *intense unpleasantness*.

Purpose and Desire.—A purpose is an idea either of an end-act, of an end-condition or consequence of an end-act, or of an object to be approached or obtained. The word *impulse* may be applied to an idea of an immediate act.

A purpose may recur often in the course of the individual's responses, provided its *stimuli* recur frequently. It is always the effect of a stimulus, and if the purpose is followed by overt action, then that action is the effect of the same stimulus. A purpose which is followed by action has had the effect of making its stimulus more likely to evoke a certain response.[2]

A purpose by itself is never a cause of sustained action.—A purpose is as transient as any response—is possibly an effector response-pattern—and has no existence, much less potency, between its occurrences. It is an event, not an entity.

An act may have occurred several times to a stimulus plus a purpose. After several occurrences, the response may become automatic—*i.e.*, occur to the stimulus alone, without intervening deliberation. For example, the first week a man goes to work he must remind himself of wages and of food-purchasing before he will rise and dress. Later, if going to work brings regular satisfaction, mere awaking is sufficient stimulus to rising, dressing, and dashing for a train. Such a response cannot then be called purposive, unless we wish to introduce confusion. A purpose is an item of direct experience.

After a purpose has been dropped in the course of learning, it *seems to function;* it has left a trace on the nervous system

[2] A neutral monism is implied in the present account. The mental event is held to function directly in a causal chain of physical changes. However, the psychological phenomena which we discuss allow equally well of expression in terms of other metaphysics.

which extends the efficacy of a stimulus-detail. Instead of assuming persistent purposes, or any other enduring internal states, we much recognize that enduring modifications of the nervous system are the only explanations for the *uniformity* of behavior, while the frequent recurrence of the proper stimuli are the only explanations for the *frequent recurrence* of certain actions.

A purpose accompanied by approval becomes a *desire* if its originating stimulus is not readily eliminated. A desire consists of a purpose plus unpleasantness and its physical correlate, restlessness. A desire is a purpose approved of, but not readily carried into action, combined with the consequent affect.

It should be noted that an affective state and unpleasantness are not identical. An *affective state*, such as fear or hunger, consists of unpleasantness *plus organic sensations*, just as a *desire* is a purpose plus unpleasantness. Both have the common affective element of unpleasantness or restlessness, but desires are differentiated into classes by their objects, and affective states by organic sensations. Hunger, for instance, is typified either by its food-object (desire) or by sensations from receptors in the stomach wall (affective state). Organic processes such as stomach contractions become the sources of stimulation leading to organic sensations, to displeasure, to the desire for a certain end, and leading also to subsequent overt behavior. So keen a thinker as Dunlap (7) writes that the entire affective basis of a desire—the 'appet'—consists in organic sensations alone, omitting mention of the really essential feeling-state. To be sure, in many desires the appet includes organic sensations as well as annoyance. But the former are not essential to the concept, nor is visceral stimulation an invariable antecedent of unpleasantness: The sight of money will cause a definite desire, without demonstrable visceral activity. We must be careful to distinguish between the viscera as source-of-organic-sensations and the viscera as hypothetical seat-of-feelings.

Definitions of Motive and Related Terms.—Except that it is vaguely connected with causation, the word 'motive' has

no uniform definition. We shall briefly review here more than a dozen meanings of the term, and still others of its derivative, 'motivation.' A list of related terms is also presented here; which, if not an exhaustive list, is nevertheless adequate to show that the nature of psychological causation has been obscured by a mass of dubious assumptions.

Other interpretations may be acceptable besides those given here. For some terms we have selected the loosest, not the best definitions.

A. Motive

a. A purpose; the conscious idea of an end-condition to be attained, which causes or seems to cause action tending toward that end; the imaginal element of a desire (**3**, *Motive*, n., def. 2).

b. Any mental state or process, conative or affective as well as cognitive, which is an antecedent to subsequent thought or behavior. (The distinction between motives as concerned with the 'intellect' alone, and motives as concerned with the will, with pleasure, and with action is a time-honored one, having been expressed by Bentham in 1789.) (**2**, pp. 97–98.)

c. An unexperienced but nevertheless mental cause of a response; a desire which has become unconscious through the process of Suppression, but continues to affect behavior from its place within the Unconscious (**16**).

d. Any enduring state or entity within the individual which explains the duration, or the frequent recurrence, of responses which do not seem to require sensory stimulation. (Motives in this sense are conceived as sufficient causes of activity—agents which eject responses outright. Purposes, desires and complexes are such motives; they cause men to go to their daily occupations, or to pursue long quests of knowledge, fortune, revenge, etc. They are general, not specific, causes of behavior.) (Popular usage.)

e. Any state or entity within the individual which determines the potency of stimuli as they are encountered, or modifies the nature of the responses they evoke; that

which selects or decides the nature of responses to a stimulus. (A commoner and more acceptable view than that of *d*. Here stimuli are held to play at least a contributing part in behavior. These motives merely 'direct' action which other agents 'arouse.') (**19**, pp. 71–72.)

f. An 'activity in progress' which is similar to Motive *e* in function. (An attempt to explain the same phenomena as *e* without implying the existence of intra-organic forces or stores of energy.) (**20**).

g. A uniform tendency toward an end which is aroused by any of a group of stimuli, and directs the individual's activity; a general goal which can be perceived in the individual's acts. (For example, the motives of self-preservation or of reproduction. Such a motive is a teleological force rather than a real mental or organic entity.) (Obsolete technically, now popular.)

h. A general rule of conduct or character trait, by which our social behavior is affected. (For example, the motives of honesty, loyalty, patriotism, or avarice.) (**2**, pp. 98–99).

i. That which gives a definite direction or form to original drives, or shapes random activity into definite patterns (**4**, p. 230 f.).

j. A generic name for an act, which is held to explain that act. (For example, the motives of imitation, play or rivalry.) (See any representative inventory of instincts, such as **19**, pp. 137, 139 ff.)

k. One of the tendencies or forces operative in a state of conflict; that which directs a choice or decision (Encycl. Brit., 14th ed., see Motive).

l. An object or situation outside the individual which attracts or repels him, causes or inhibits action. (As distinguished from the experience of such an external object, or the stimulation caused by such an object.) (**2**, p. 99; **3**, *Motive*, n, def. 1, note.)

m. The specific cause of a single response; more precisely, a stimulus. (This use makes the term precisely syn-

onymous to *stimulus*. A present-day definition, in conformance with a strict stimulus-response interpretation of mental events (**11**, p. 304; **17**, p. 443).

n. A persisting or recurring stimulus, one which by its nature is more than momentary. (Examples given are hunger and excessive light.) (**11**, p. 308.)

B. Motivation

a. The relation between cause and effect; causation; in psychology, the relation between stimulus and behavior (**11**, pp. 300–304).

b. The functioning of the energies or forces behind reactions (**4**, p. 230); the functioning of the entities or states behind reactions, such as desires, purposes, etc. (Popular.)

c. The process of directing energy; the shaping of behavior (**4**, p. 230 f).

d. The increasing of efficiency or speed of learning, by the arousal of interest or effort; the cause of such an increase. (A common topic in educational psychology, in connection with lesson planning and classroom procedure.) (**8**; **4**, p. 230.)

e. The process or art of arousing men or instigating action (**3**, see *Motivation*).

f. An entity or force, external or within the individual, which adds energy or force to a reaction; an incentive; the actual phenomenon of increase in energy or forcefulness. (Not only can a motive cause an act, but an act going on may be pepped up by 'providing motivation.') (**4**, p. 229.)

C. Related Terms

Drive:—A prepotent habit or group of habits (1); a tissue condition giving rise to stimulations which cause overt activity (**4**, p. 233 f.); stored chemical energy in receptors, neurons, and effectors (**12**, p. 167); a stimulus (**17**, p. 185 f.); an entity partaking variously of the qualities of Motives *d*, *e*, *f*, and *m*, in Woodworth's historic organization of the topic (**18**); an original source of

energy or activity—the analogue to motive power in an engine (**4**, p. 273); most commonly, an entity identical with Motive *d* or *e*. See also *Motives l* and *i*, and *Motivation f*.

IMPULSE:—The kinesthetic perception of an uncompleted movement (**11**, p. 369 f.); an experienced tendency, or striving, to perform an act which has been stimulated but not yet expressed (**19**, p. 155).

URGE:—See *Impulse*. (May be unconscious.) See also *Motive d* (**14**).

MENTAL ENERGY:—An unknown psychic force assumed to explain certain mental events (**15**).

LIBIDO:—See *Mental Energy*. (Apparently a fluid.) (**10**, pp. 2–6.)

UNCONSCIOUS:—Locality or sum of suppressed but active complexes (**10**, pp. 24–28). Alias the Id (**10**, p. 36).

PURPOSE; DESIRE; COMPLEX; INSTINCT; INCENTIVE;—See *Motive d*. Also applies to *Habit*, used loosely. For Incentive, see also *Motivation f* and *Motive l*.

SET; INSTINCT; ATTITUDE; INTEREST; SENTIMENT; DISPOSITION; TRAIT; CHARACTER;—See *Motive e*. In general, a tendency to react in a certain way. Interest is also an element of experience. Set and Attitude may refer to muscular contractions.

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QUOTIDIAN VARIABILITY

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It is commonly recognized that experimentation is more difficult in psychology than in the physical sciences because of the fact that in the psychological experiment it is impossible completely to control all the conditions. Typically, a psychological experiment consists of a description or measurement of the reactions of an individual placed in a predetermined environmental situation. The control of the latter may not be particularly troublesome, but the control of the internal condition of the subject is never complete and often is so loose as to permit of highly significant variations.

A certain amount of variation within the subject is regularly to be expected. As a result of this variation, the subject yields responses the measurements of which are distributed over a considerable range of values. If it is desired to compare an individual's reactions under two different sets of external conditions, it is necessary, therefore, to consider not merely a few reactions obtained under each of the two sets of conditions, but two total distributions of reactions. It has been customary to assume that it is possible to determine whether any change introduced into the external conditions has had a significant effect by making use of the standard deviation of the difference between the central tendencies of the two distributions, calculated by the formula, $\sigma_{diff.} = \sqrt{\sigma^2_{M1} + \sigma^2_{M2}}$. If the difference exceeds three times its standard deviation, it is taken to mean that the introduced change in external conditions may be depended upon as exerting an effect upon the subject's behavior. As a matter of fact, however, this may not be at all a valid conclusion if the internal condition of the subject was not the same under both sets of external conditions; and the indications are, as will be pointed out below, that in a large proportion of carefully

conducted psychological experiments these internal conditions are so poorly controlled that a difference between the averages of two sets of responses, each including perhaps 50 or 100 measurements, may, and very frequently does, far exceed three times its standard deviation even though both sets of responses were obtained under the same external conditions and with the same instructions.

The cases in which this significant variation under supposedly constant conditions has been particularly studied by the writer are cases in which the separate sets of responses were each obtained at a different sitting. It is such variation from sitting to sitting, or from day to day, here designated by the term, 'quotidian variation,' that is to be considered. A method of measuring this characteristic, yielding what may be termed an index or ratio of quotidian variation will be described, and illustrative results obtained by its use will be presented. It is believed that this index may be of significance in the description of individuals—possibly even in clinical psychology—since under the same test conditions individuals differ greatly in the degree of instability of behavior from day to day. It is probable, however, that a more important use for this index lies in the fact that a recognition of the conditions revealed by it should be conducive of greater efforts in psychological experimentation to control the internal condition of the subject, whether by more adequate instructions or otherwise, and should lead to an actual determination of whether the condition of the subject has been controlled to the point where significant differences do not occur under supposedly constant conditions. Efforts along these lines should result in improving the verifiability of the conclusions drawn from a psychological experiment, a verifiability which has been notably uncertain in many instances in the past.

When successive measurements of any psychological response are made under constant external conditions with fixed instructions to the subject, it is ordinarily assumed that the measurements may all be regarded as of the same universe or category. Now one consequence of the statistical theory of such measurements is the widely used formula, $\sigma_{av.} =$

$\sigma_{dis.}/\sqrt{n}$. From this formula it follows that in the case of measurements made at a *number* of sittings the variability of a subject as measured by the $\sigma_{dis.}$ of any one day should, when divided by $\sqrt{n}$, approximately equal the σ of his daily averages, provided the subject's reactions remain from day to day reactions of the same category. In other words, apart from the change day by day in the subject, here termed his quotidian variation, the experimentally obtained σ of the daily averages (obt. $\sigma_{av.'s}$) should equal the average daily σ divided by $\sqrt{n}$, (av. $\sigma_{dis.}/\sqrt{n}$); and the ratio of the first value to the second should be unity. This ratio is termed the index of quotidian variation. Of course probable errors or mean variations may be used in place of standard deviations. It is assumed that before calculating this ratio a correction has been made in both the averages and the standard deviations for any factor, such as practice, which tends to produce any deflection from constancy in the *general* trend of the averages.

It is certainly true, if the measurements obtained at each sitting may be looked upon as analogous to numbers drawn in sets from one very large normally distributed mass of numbers, that the formula $\sigma_{av.} = \sigma_{dis.}/\sqrt{n}$ is satisfactory. It may easily be verified experimentally. Thus, using the data supplied by Thurstone,[1] who drew 50 samples of 20 each from a pile of 1000 numbers showing a normal distribution with a standard deviation of 4 and an average of 12, it may be calculated that the obtained $\sigma_{av.'s}$, that is, the standard deviation of the averages of the separate samples, is .92, while the average $\sigma_{dis.}/\sqrt{n}$, that is, the average of the fifty standard deviations divided by $\sqrt{20}$, is .85. If the separate draws of 20 each be regarded as the equivalent of separate experimental sittings, yielding 20 measurements each, then it may be said that the data of Thurstone's experiment yield an index of quotidian variation of .92/.85 or 1.08, a ratio which does not differ significantly from unity. The purpose of the present paper is to study the question: To what extent are the measurements obtained at separate sittings under supposedly constant conditions analogous to numbers drawn at random from one large dis-

[1] L. L. Thurstone, Fundamentals of statistics, 1925, p. 169.

tribution of numbers? In short, the question is, to what extent are the measurements obtained at different sittings measurements of the same category, or universe, of responses? The answer consists in a determination of how nearly the indices of quotidian variation approach the theoretical value of unity.

In the experimental data so far investigated, the above mentioned statistical expectation of an index in the neighborhood of unity has not been verified. The index of quotidian variation is generally higher than unity, often significantly so. It is not infrequently as high as 4 or 5, even when the subject has been carefully instructed and the subject himself has reported that he was doing the same thing day after day. In the cases so far examined by the writer the relationship varies with individuals from that approaching the theoretical ratio of 1 to a ratio of over 7.

A good illustration is afforded by some data recently published by Thorndike in his book on *Human Learning*. He presents these data in answer to the following question: "What would happen if a man could be subjected to the same situation, say 1,000 times, with everything else in the world and in him kept constant save the thousand repetitions of the situation and the changes if any, which they produce in him?"[2]

The data consist of measurements of 2203 lines drawn by Thorndike himself, with his eyes closed, in an attempt to draw a four-inch line with one quick movement, on the same pad, in the same position. The trials were obtained at 12 sittings with from 171 to 200 trials at each sitting. The data illustrate two principles, according to Thorndike: (1) that of multiplicity of responses and (2) that of the failure of mere repetition to cause learning. In mentioning the multiplicity of responses, Thorndike does not discuss the significance of the differences between sittings, but only the general fact of variability. This he attributes in general to 'subtle differences in the brain and nerves and muscles of the individual from minute to minute which cause multiplicity or variety of responses to the same external situation.'

[2] E. L. Thorndike, Human learning, 1931, p. 8.

The data clearly indicate, however, that these variations which occur from minute to minute, which may be termed chance variations, are superimposed upon variations of a different sort which occur from day to day. The standard deviation of the daily averages (obt. $\sigma_{av.'s}$) is .127 inch, whereas the average daily $\sigma_{dis.}/\sqrt{n}$ is only .023 inch. The ratio of these two values, the index of quotidian variation, which, had the responses all belonged to the same category, should be approximately one, turns out to be 5.6, with a probable error [3] of 1.11 or slightly less than 1/5 of the ratio. The responses on different days clearly are not all of the same category; they belong to different statistical populations. Evidently, if the external conditions were constant, Thorndike did not succeed in the task, which he set out to accomplish, of keeping everything within the subject constant.

There are other methods, based upon somewhat the same general principles as the index of quotidian variation as defined above, which lead to substantially the same conclusion. In particular, one might be inclined to use the average of the differences between the daily averages, and to compare the obtained average difference with that to be expected upon the hypothesis that each day's measurements represent a random sampling from the total population composed of all the measurements obtained in the entire series of sittings. Such a method would represent an extension of the ordinary custom of deciding the question of the significance of a difference between any two averages by dividing the difference by the standard deviation of the difference. In the case of Thorndike's data, the first day's average differs from that of the third day by 15.2 times the $\sigma_{diff.}$, and from that of the last day by 9.3 times the $\sigma_{diff.}$ This procedure, however, does not give any one measure of that trait of the individual which

[3] The probable error of the index of quotidian variation, which is a ratio, has been calculated by the formula, $\text{P.E.}_{B/A} = .6745\sqrt{\left(\frac{Ba}{A}\right)^2 + b^2}/A$, in which B and A are the numerator and denominator of the ratio, and b and a the standard deviations of B and A. In calculating the standard deviation of the numerator, which is itself a standard deviation, the formula $\sigma\sqrt{2n}$, in which n is the number of sittings, has been used. The standard deviation of the denominator has been taken as the obtained standard deviation of the standard deviations of the average of the daily sittings.

it is the aim of the quotidian ratio to describe, that is, the variation shown from day to day by the subject in the daily sittings taken as a whole. To arrive at a measure of this trait it would be necessary to consider all the obtainable differences between the daily averages. In the case of Thorndike's twelve sittings, the number of obtainable differences between daily averages is 66; and these 66 differences yield an average difference of .16 inch. The question then becomes this: is the average of the differences between the daily averages greater than that to be expected upon the assumption that the cases composing any one day's sitting represent a sample drawn at random from a normally distributed population? The average difference to be expected may be calculated from the formula, $2\sigma/\sqrt{\pi n}$ or $1.1284\sigma/\sqrt{n}$, in which n is the number of cases comprised by one sample and σ is the standard deviation of the total population of measurements.[4] When this formula is applied to a set of averages obtained by random sampling, the theoretical average indicated by it will be approximately equal to the average difference between the obtained averages. Thus, in the case of the experimental drawings made by Thurstone, to which reference has been made above, the obtained average of the 1,225 differences between the 50 averages is 1.01, whereas the average calculated by the above formula is 1.02, *i.e.*, $\frac{1.1284 \times 4}{\sqrt{20}}$. On the other hand, similar computations applied to Thorndike's data yield results as follows: obtained average of the 66 differences between 12 daily averages, .16; theoretical average, by formula, .028. The ratio of the obtained average difference to that to be expected if all the measurements belonged to one population is, therefore, .16/.028 or 5.7. This is practically the same as the ratio of quotidian variation, 5.6, given above, calculated from the formula, obt. $\sigma_{av.'s}$/(av. $\sigma_{dis.}/\sqrt{n}$). On the whole the latter formula represents the better method. To calculate the average of the differences between all pairs of measurements is analogous to using as a

[4] This formula was kindly derived for the writer by Professor A. R. Crathorne, of the department of mathematics of the University of Illinois.

measure of dispersion not the generally accepted σ or P.E. but the average of all the differences between each measurement and all the others. Such a procedure is so much at variance with existing custom that it would be confusing. As a rule, moreover, it is very laborious. The main purpose in describing it here is to show that it leads to substantially similar ratios as the formula employing only σ's and n.

The index of quotidian variation may also be calculated with a fair degree of accuracy even in the case where the experimental conditions are changed from day to day, provided that a large enough group of subjects has been used to determine the effect of the experimental changes. If the effect of the introduced changes in experimental conditions is known, the averages and standard deviations may be corrected for this effect and the index of quotidian variation therefore calculated with the effect of the experimentally varied conditions removed. An illustration of this procedure may be given from data recently obtained by the writer on the accuracy of synchronization to auditory stimuli given at various rates, but at only one rate at any one sitting.[5]

The data for two illustrative subjects, in Table I, show the average error made in attempting to produce by tapping a key a series of sounds which would synchronize with a stimulus-series run off at a uniform rate. Each average, or constant, error (C.E.) was calculated as a percentage of the interval between the sounds of the stimulus-series. The percentage constant errors showed little, if any, effect of rate, that could be regarded as significant, but were nevertheless corrected for the effect of rate by assuming that this effect was represented by the variation in the average of the constant errors of all subjects. Upon this assumption the effect of rate would be eliminated by multiplying all the constant errors obtained with any one rate by whatever number was necessary in order to change the average constant error at that rate to the average for all rates. For example, the average error for all rates was -1.275 per cent. For the rate of

[5] For a complete account of this study, see H. Woodrow, The effect of rate upon accuracy of synchronization. *J. Exper. Psychol.*, August 1932, 15.

one sound every .250 sec it was − 1.8 per cent. Consequently the C.E. of each subject at the .250 sec rate was multiplied by − 1.275/− 1.8, or .71. The result obtained is called the constant error with the effect of rate eliminated. Each standard deviation of the distributions was, of course, treated exactly like the averages, *i.e.*, after being reduced to a percentage of the rate, it was multiplied by the same number as that by which the corresponding average was multiplied. It is these corrected constant errors and their standard deviations that are given in Table I.

TABLE I

CONSTANT ERRORS AND STANDARD DEVIATIONS OBTAINED FROM TWO SUBJECTS AT SIXTEEN SITTINGS IN A SYNCHRONOUS TAPPING EXPERIMENT

C.E.'s and σ's are given as percentages of the rate, after elimination of the effect of rate.

Under rate is given the interval between any two successive stimulus-sounds in sigma.

		Subject Bl.		Subject Mn.	
Day	Rate	C.E.	$\sigma_{dis.}$	C.E.	$\sigma_{dis.}$
1	250	− 8.4	7.4	− 5.4	9.2
2	400	− 2.5	4.9	− 1.3	6.0
3	571	− 7.7	5.3	− 5.6	5.9
4	667	− 4.3	5.7	− 6.6	6.3
5	800	− 3.6	5.2	− 2.8	6.1
6	1000	− 2.9	4.7	− 2.1	5.7
7	2000	+ 0.5	5.5	− 1.4	4.8
8	4000	+ 2.1	3.8	− 0.3	5.9
9	4000	+ 2.1	3.7	− 2.4	6.1
10	2000	− 1.2	4.3	−2 .3	5.3
11	1000	− 2.0	5.6	− 1.0	5.5
12	800	− 2.6	5.5	+ 1.0	5.1
13	667	− 3.5	5.3	+ 1.3	4.7
14	571	− 8.9	5.3	+ 1.3	5.1
15	400	− 10.5	6.6	+ 1.4	3.9
16	250	− 0.9	4.2	+ 5.8	3.2
Obt. $\sigma_{C.E.'s}$		3.7		3.0	
Av. $\sigma_{dis.}$			5.2		5.6
Av. $\sigma_{dis.}/\sqrt{n}$			.74		.79
I.Q.V.		3.7 / .74 = 5.0		3.0 / .79 = 3.8	

In the case of subject Bl., the daily C.E.'s show a standard deviation (obt. $\sigma_{av.'s}$) of 3.7, whereas the average daily $\sigma_{dis.}$ divided by $\sqrt{n}$ is only $5.2/\sqrt{50}$ or .74. The ratio of quotidian

variation is therefore 3.7/.74 or 5.0, with a P.E. of 0.81. Similar computations for subject Mn. yield a quotidian ratio of 3.8, with a P.E. of 0.75. The indices of quotidian variation calculated from the uncorrected C.E.'s are slightly larger, being 5.2 and 4.4 as compared with 5.0 and 3.8. These ratios are interpreted to mean that the variation shown by these individual subjects in their C.E.'s cannot be attributed to the effect of rate as such; it is rather to be explained as due to shifts in attitude, or internal condition. In the present instance, it is not definitely known what shifts in attitude cause a change in the sign or in the magnitude of the C.E.'s. A great many conceivable causes are indicated, however, by the subjects' introspections. Among the attitudinal characteristics in which a given subject varied from one sitting to another, according to these introspections, are the following: variation in mode of apprehending the sounds, e.g., as a unified or rhythmical series or as a series of separate taps; variation in strain sensations from hand, wrist, shoulder, larynx, eyes and brow and in breathing, or variation in the degree of attention to these; counting or not counting; variations in confidence, fatigue, general alertness; differences in the degree to which tapping was what the subjects called automatic and what they called voluntary; variations in the type or force of the tapping movements; variation in the distribution of attention between the tapping movements (or sensations therefrom) and the sounds; variation in the effort, and the promptness with which this effort was made, to get back into time again upon observing a lack of synchronization, *i.e.*, variation in the tendency towards perseveration in an adopted rate even after it has led to observable lack of synchronization; etc.

The main value of such introspections is to suggest further types of instructions. These different types of instructions must be tried out to determine their effect, if any, upon the obtained averages. Such work is slow because many variations in instruction have little or no measurable effect. Only a few tasks have been analyzed to the point where certainty exists concerning the effect of even gross differences in instruc-

tion upon performance. One of these tasks is the simple reaction; another is the task of reproducing empty intervals. With respect to the latter task, it has been found that it is possible by special instructions to obtain either over-estimation or under-estimation with either very short or very long intervals.[6]

The writer has not yet found a series of measurements which show a quotidian variation as low as the theoretical ratio of 1.0. In examining the results of one subject whose reaction time was measured 30 times a day for 23 days, a quotidian ratio of 3.3 was obtained.[7] In the case of reproductions of empty time-intervals, the size of the index of the quotidian variation is striking. In fact it was the great inconstancy of the constant errors of any one subject in the reproduction of intervals that first led the writer to analyze more thoroughly this matter of variation from day to day. Subjects were found to show statistically reliable positive constant errors on one day and on another, perhaps with the same interval, to show reliable negative errors. Results for two subjects will suffice to indicate the general trend in this matter. Eight subjects, who, in 22 sittings, reproduced intervals varying from .200 sec to 10.0 sec, gave results which were regarded as representing the effect of the duration of the stimulus-interval upon the errors of reproduction, the latter being calculated as percentages of the interval. At each sitting 50 reproductions of one interval were made; and since the intervals were used first in an ascending and then in a descending order, the work with the 11 intervals was divided into 22 sittings, each occurring on a different day, and each yielding as measures of a subject's performance an average and a standard deviation. The intervals used, in seconds, together with the average percent of error shown by the reproductions of the eight subjects were as follows:

[6] H. Woodrow, The reproduction of temporal intervals, *J. Exper. Psychol.*, 1930, 13, 496–497.

[7] For the data, see H. Woodrow, The measurement of attention, *Psychol. Monog.*, 1914, 17, No. 76, p. 130, Table XVII, subject Lm, 1st Series. Since only MV's are given, in this case the I.Q.V. was calculated by dividing the obtained MV of the 23 daily averages by the average of the daily MV's divided by $\sqrt{n}$.

Interval.....	.2	.4	.6	.8	1.0	1.2	1.5	2.0	4.0	6.0	10.0
C.E. (%)....	+6.8	+4.4	+4.3	+3.8	+2.5	+2.0	+2.3	+3.5	+4.3	+5.5	+9.2

The average of all the constant errors is + 4.4 percent. To eliminate the obtained effect of length of interval the constant errors and standard deviations of each subject with each interval were multiplied by the figure obtained by dividing 4.4 by the average C.E. for that interval, so that the average of the corrected C.E.'s would in the case of each interval be the same, namely, 4.4. The results thus obtained for two subjects, are given in Table II.

TABLE II

AVERAGE ERRORS (C.E.'s) AND STANDARD DEVIATIONS IN THE REPRODUCTION OF TEMPORAL INTERVALS AT TWENTY-TWO SITTINGS, AFTER CORRECTION TO ELIMINATE THE EFFECT OF LENGTH OF INTERVAL

The C.E.'s and σ's are given as percentages of the interval.

Interval in Sigma	Subject Fn		Subject Ke	
	C.E.	σ	C.E.	σ
200	− 0.8	6.7	+ 10.0	10.5
400	− 6.5	8.2	+ 12.8	10.2
600	+ 2.6	7.4	+ 13.9	5.0
800	+ 4.5	7.5	+ 20.7	4.8
1000	− 6.2	10.5	+ 8.3	11.4
1200	+ 4.2	16.7	+ 9.0	10.6
1500	+ 11.9	14.3	+ 9.1	12.2
2000	+ 9.7	12.6	− 4.7	7.8
4000	− 8.2	10.8	− 9.3	9.5
6000	− 3.6	13.8	+ 3.8	11.5
10000	+ 4.8	11.8	− 5.6	7.1
10000	+ 10.1	8.5	+ 10.9	8.6
6000	+ 4.7	13.8	+ 20.1	11.7
4000	+ 5.5	13.9	+ 21.3	15.8
2000	+ 1.5	14.2	+ 5.2	4.0
1500	− 1.9	20.0	+ 13.3	13.7
1200	− 13.8	22.4	+ 37.0	24.9
1000	+ 0.2	10.5	+ 28.9	13.7
800	− 5.4	9.5	+ 25.0	6.3
600	− 5.3	7.4	+ 31.4	11.1
400	− 10.7	15.9	+ 27.9	10.9
200	+ 0.5	5.3	+ 17.2	6.8
Obt. $\sigma_{av.'s}$	6.7		11.2	
Av. $\sigma_{dis.}$		11.9		10.4
Av. $\sigma_{dis.}/\sqrt{n}$		1.68		1.47
I.Q.V.	6.7/1.68 =	4.0	11.2/1.47 =	7.6

Fn's 22 corrected percentage C.E.'s show a standard deviation (obt. $\sigma_{av.'s}$) of 6.7; whereas the deviation obtained

by dividing the average $\sigma_{dis.}$ for one sitting by $\sqrt{n}$ was only 1.68. The index of quotidian variation, then, was 4.0. For Ke, the results were as follows: obt. $\sigma_{av.'s}$, 11.2; average $\sigma_{av.'s}$ 1.47; index of quotidian variation, 7.6.

The results cited above indicate that in the average psychological experiment pertinent conditions, probably for the most part conditions within the subject, are not adequately controlled. Under conditions such as ordinarily prevail the processes which go on in the subject and lead to the measured responses change to such a degree that the measurements made on one day may not belong to the same population or category as those made on a different day, even though all conditions that are controlled by the experimenter remain constant. It is due to this fact that the Brown-Spearman reliability coefficient based on the correlation between odd and even scores obtained at the same sitting is greater than the correlation between scores obtained at different sittings. Odd and even scores from one sitting are based on results obtained very nearly under the influence of the same internal conditions, whereas scores obtained at two different sittings are subject to the effect of shifts in attitude and other changes in the internal condition of the subject. The effect of these changes in the subjects from one sitting to another upon correlation has been emphasized by Paulsen,[8] who has proposed a 'coefficient of trait variability' which measures the variability of a *trait* from one sitting to another in the form of a corrected correlation coefficient, calculated of course from results obtained from a group of persons. The present paper describes a different kind of measure termed an index of quotidian variation, which affords a needed supplement to Paulsen's coefficient, and which, since it can be calculated from the data afforded by even a single subject, constitutes a measure which may be used not only for the study of relative stability, from sitting to sitting, of performance in the case of different tasks, but also for the study of the stability of different individuals.

[MS. received October 22, 1931]

[8] G. Paulsen, A coefficient of trait-variability, Proc. Amer. Psychol. Ass'n., *Psychol. Bull.*, 1931, 28, 218–219.

MATERIALIZING THE GHOST OF KÖHLER'S GESTALT PSYCHOLOGY

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What is a *Gestalt?*

According to Wolfgang Köhler, "In the German language the noun *Gestalt* has two meanings: besides the connotation of 'shape' or 'form' as a *property* of things, it has the meaning of a concrete, individual, and characteristic entity, existing as something detached and *having* a shape or form as one of its attributes. Following this tradition, in *Gestalt theorie* the word *Gestalt* means any segregated whole, and the consideration of *Gestalt-qualitäten* has become a more special side of the *Gestalt problem*, the prevailing idea being that the same general type of dynamical process which leads to the formation and segregation of extended wholes will also explain their specific properties." [1]

While it is not entirely fair to lift a definition of the term *Gestalt* bodily out of the setting in which it is contained and detach it from the wealth of illustration with which it is usually accompanied in Gestaltian literature, yet when one goes to any of Köhler's writings in English (to which this paper confines itself), one is left almost as baffled to discover palpability in this elusive Gestaltian ghost as in the detached definition itself.

Nor does one find reassuring tangibility in Koffka's definition of a 'configuration,' his synonym for a *Gestalt.* Here is his description: "It is a part of the nature of a quality that it should lie upon a ground, or, as we may also say, that it should rise above a level. Such a coexistence of phenomena in which each member carries every other, and in which each member possesses its peculiarity only by virtue of, and in connection with, all the others, we shall henceforth call a configuration." [2]

[1] W. Köhler, Gestalt psychology, pp. 192–193.
[2] K. Koffka, Growth of mind, pp. 131–132.

As one reads the literature of the Gestalters, one is impressed with the possible reality of an elusive 'something' which they call a *Gestalt*. But no one seems to have defined the term clearly and about it everyone grows vague and mystic when urged to explain it. If in doubt about this statement, ask almost any psychologist the simple question, What is a *Gestalt?* and try to follow his attempt to reply.

The substance with which it is proposed in this article to materialize the Gestaltian ghost it nothing more nor less, so far as the field of consciousness is concerned, than the kinesthetic experiences accompanying the taking of bodily attitudes toward objects. These kinesthesias constitute the basis of an idea, according to the Washburnian motor theory of consciousness, although every idea is furnished with a distinguishing *quale* by contributions from one or more sensory or imaginal sources other than the essential kinesthetic ones. Outside the field of consciousness, a *Gestalt* is an equilibrium of bodily adjustments to an object or situation.

To state the motor theory of consciousness a little more exactly for purposes of applying it to the materialization of *Gestalten*, it is declared by Miss Washburn that "consciousness depends on a certain ratio between excitation and inhibition of the motor response's being sufficiently innervated to be actually performed, though only in the weakened form of a tentative movement." [3] By a 'tentative movement' is meant "the slight actual movements, peculiar to a given stimulus, which accompany attention to that stimulus." [4]

Professor Langfeld, in his presidential address before the Iowa City meeting of the American Psychological Association in December, 1930, speaking in defense of the motor theory of consciousness under the title *A Response Theory of Consciousness*, quotes from Miss Washburn as follows: "Now the reactions themselves, of whatever nature, give rise to kinesthetic excitations, and this whether these movements are fully performed or are incipient, tentative movements only. These kinesthetic excitations are a regular accompaniment of

[3] M. F. Washburn, Movement and mental imagery, p. 36.
[4] M. F. Washburn, Movement and mental imagery, p. 48.

such excitations from outside stimuli as are reacted to at all; they influence all conscious processes. *Whether they themselves are directly represented in consciousness depends on whether they in their turn are reacted to.*" [5]

To elaborate the motor theory of consciousness still further in order to make it clearer, it may be said that to have an idea is to have an appropriate incipient bodily attitude, both visceral and somatic, as the basis of the idea. To imagine is to assume the original tentative attitude toward the thing imagined, which attitude in turn evokes contributions to consciousness from other associated imaginal centers. To remember is to enrich imagination with personal attitudes that give consciousness of the self as it was in the original setting. To perceive an object is to take appropriate incipient responses to that particular object. To conceive is to reassume only the *elements* of the incipient responses common to all the attitudes ordinarily taken toward the known individuals of a class or group of objects. To sense the meaning of an object is to be aware of the use of the object, which awareness rests in turn on the assumption of appropriate incipient adjustments to the object if it were in use. To judge is to sense agreements or disagreements between two objects in consequence of taking incipient attitudes appropriate to each of them. To reason inductively is to arrive at a new concept in consequence of the emergence of an awareness accompanying excitement of the common elements of attitudes in the course of perfecting a previously imperfect concept. To reason deductively is, in its last analysis, to discover that the essential attitudinal elements in the minor concept or percept are in the middle concept, and these in turn are in the major concept, when these attitudes are translated into ideational terms.

For Miss Washburn, "All association is the association of movements." [6] To think contiguously is to evoke idea-attitudes previously associated in experience with other idea-attitudes. To think in similars is to evoke idea-attitudes by

[5] H. S. Langfeld, *Psychol. Rev.*, 1931, 38, p. 99.

[6] M. F. Washburn, Movement and mental imagery, p. 88

other idea-attitudes not previously juxtaposed in experience but drawn together by the excitation of *common elements* of attitudes. To attend is to increase the tension of the muscles involved in the tentative movements of an idea. Ideo-motor action is attention to an idea until its undergirding movements pass over from tentative to overt movements. Volition is ideo-motor action in which the association is with the central activity system, that is, with the core of all movement systems that make up one's behaving personality.

This paper is not designed to establish the validity of the motor, or response, theory of consciousness. However, to escape the suspicion of pure dogmatism, two lines of argumentive facts are here merely indicated in support of that theory.

The first set of facts has to do with commonly observed experiences such as make it appear as if consciousness is a function of movement, whatever else it may be. Only an abbreviated catalogue of common phenomena is here presented, namely: fatigue in relation to sleep and to thinking; the whole field of ideo-motor action, such as the tendency to help a faltering automobile up a hill, one's behavior in helping a football team hold the line, and the pedal attitudes of a crowd watching a pole vaulter as he goes over; all cases of muscle (mind) reading; the feeling that one has forgotten something to be done; sensing 'volume' in noise or sound; the phenomena of body and arm movements in animated speech; children's definitions in terms of use; differences in the vividness of sensations and images; Helen Keller's rare mental ability; the phenomenon of perseveration; the whole realm of orientation experiences; and the neurology, ontogeny, and phylogeny of the brain. All these phenomena seem to be explained easily on the motor theory of consciousness but most difficultly on any other hypothesis.

People who have considered the motor theory of consciousness and rejected it, or have been doubtful of its validity have usually done so because they have been unable to discover any sign of special bodily attitude or adjustment in case of entertaining concepts, particularly abstract ones, and

even of many ordinary percepts. To meet this very natural objection a second important set of argumentative facts has recently been brought to light. This most convincing evidence is found in the work of Dr. Edmund Jacobsen, of the University of Chicago, as reported in a series of papers on *Electrical Measurements of Neuro-Muscular States During Mental Activities.*[7] This evidence can easily be examined by any interested reader.

Without trying further to expand the motor, attitudinal, or response theory of consciousness, the attempt is here made to inject the materializing fluid of idea-attitudes into the shadowy veins of ghostly *Gestalten*, particularly as that ghost walks through the pages of Köhler's writings that have appeared in English.

In *The Psychologies of 1925* we have Köhler's first contribution in English for American readers. His article is entitled *An Aspect of Gestalt Psychology*. Here Köhler opens by saying, "What we call 'Gestalt Psychology' means a new point of view and a new procedure in various respects and in several parts of psychology, so that it is far beyond my power to give a complete and adequate idea of it in one lecture." [8] What Köhler then proceeds to do is to confine his discussion to *Gestalten* in the visual field. His emphasis here as everywhere is on the *whole* of an experience rather than on its segregated, or isolated, sensational elements.

Too much credit can scarcely be given to the Gestaltists for their emphasis of the importance of the subtler elements of a conscious experience—elements which make the 'whole greater than its parts.' But in all their discussion of sensational elements, they rarely take cognizance of kinesthetic sensations and their possible contributions to the consciousness of the meaning of the whole object.

In nearly all of Köhler's writings there is the usual presentation of dots, bars, diagrams, and figures assembled to show how we see objects in groups and various *Gestalten*,

[7] E. Jacobsen, *Amer. J. Physiol.*, 1929, 91, pp. 567–607; 1930, 94, pp. 22–34; 1930, 95, pp. 694–712.

[8] Psychologies of 1925, p. 163.

all of which he offers as elements in the experience that the ordinary psychologists quite disregard. While Miss Washburn has lucidly shown [9] that this tendency to grouping is in consequence of one's taking incipient motor adjustment to the several groups or hidden figures, yet Köhler in his *Gestalt Psychology* [10] quite rejects Miss Washburn's proposed explanation.

Summarizing his presentation of visual *Gestalten* Köhler says: "The fact that not the local properties of given stimuli but the relation of those properties to each other (the total constellation of stimuli, to use a better word) are decisive for the formation of units, suggests at once the idea that *dynamic intercourse* in the field decides about what becomes a unit, what is excluded from it, what is 'figure,' and what falls back as mere 'ground.'" [11]

But what is the ghost 'dynamic intercourse' if it is not the adjusting of movement systems? The nearest that Köhler comes to letting us know the medium in which 'dynamic intercourse' operates, is found in this sentence: "Of course there are conductors in the theoretical ideas of *Gestalt* psychology, too, but they play a very different rôle here, being—in a sensory field, for instance,—a rather indifferent quasi-homogeneous network, which in itself does not prescribe what the outcome of nervous dynamics shall be or where a process shall go." [12] Further on he says: "Reorganization of the field by subjective stress, if the field is not too stable, seems to be an important side of intelligent behavior." [13]

In the statement just quoted, Köhler refers us to another ghost, 'subjective stress,' so diaphanously constituted as to present no substance upon which to fixate the eyes of the mind. If only into this mysterious ghost we can inject the opaque substance of actual motor adjustments, our ghost will be found to be a real personality even if occasionally it is con-

[9] M. F. Washburn, *Amer. J. Psychol.*, 1926, 37, pp. 516–520.
[10] W. Köhler, Gestalt psychology, pp. 166–167.
[11] Psychologies of 1925, p. 179.
[12] Psychologies of 1925, p. 186.
[13] Psychologies of 1925, p. 195.

cealed within the rather tenuous raiment of imperfect definition.

As a background for the consideration of *Gestalten* Köhler pays his respects to what he conceives to be behavioristic psychology and admits that the physiologist can render assistance to the psychologist when he is able to measure visceral changes paralleling emotional states. If Köhler could have brought himself to accept the modernized James-Lange theory of emotions, he would already have taken the initial step toward the understanding and appreciation of the motor theory of consciousness in the cognitive realm, for it quite duplicates the processes of affective and emotional experience assumed in the James-Lange theory, which is essentially that consciousness *follows* internal changes, not *precedes* them. Indeed, Köhler goes so far in this direction as to say that "we also have some evidence for assuming that, in addition to the reactions just mentioned, so-called 'thinking' consists in slight innervations of the muscles concerned in speech reactions." [14] If Köhler could extend his 'slight innervations' to responses quite beyond the laryngeal region, he would have in hand the trap to use in capturing and materializing his evanescent *Gestalten*.

One of Köhler's most significant discussions has to do with his 'objective experience,' his world of direct or immediate experience. His arguments for the reality of this experience [15] lack only a definition of its actual nature to make it quite convincing.

Had he accepted certain findings in Dr. C. H. Judd's presidential address before the American Psychological Association on *The Place of Consciousness in Evolution*,[16] he might have put more realism into his *Gestalten*. For Judd, the third great advance in evolution was that in which a creature became able to take his environment into himself and work it over so as to compel it to fit his needs. This taking in of one's environment into one's self conceivably consists in assuming the appropriate tentative attitudes to the various

[14] W. Köhler, Gestalt psychology, p. 19.
[15] W. Köhler, Gestalt psychology, p. 20.
[16] C. H. Judd, *Psychol. Rev.*, 1910, 17, pp. 77–97.

features of the environment. In this way several such objects can be taken into one's consciousness and worked over to suit the future needs of the individual. The world of 'direct experience' and introspection thus becomes a real world not only for the attitudinal psychologist but for the behaviorist, the mentalist, and the Gestaltist, as well as for the physicist.

Köhler has attempted to narrow down an introspectionist's interpretation of things to the point of excluding the important element of meaning, and accuses both introspectionist and behaviorist of being enslaved to the 'constancy hypothesis'—a constant relation between local stimulation and local response. We are told that "meaning transforms sensations into 'things.'" [17] But what is the mystic 'meaning' that performs this feat? For the motor theorist it is the consciousness resting on the usual response attitudes toward the thing from which sensations arise, whether that thing be a man who is a yard or a mile away, or a plus sign with its adding attitude seemingly localized in the visual field.

In the further discussion of the 'constancy hypothesis' Köhler declares that "instead of reacting to local stimuli by local and mutually independent events (the machine theory) the organism reacts to an actual *constellation* of stimuli by a total process which, as a functional whole, is its responses to the whole situation (the dynamic theory). . . . For the sake of order in nervous function the machine theory excludes organization of process in the field." [18] What is proposed as an escape from the rigid confinement of purely inherited or acquired arrangements is *dynamical self-distribution.*

Now it seems to some of us that 'dynamic self-distribution' could take tangible form if we think of the human mechanism not as a simple, one-way-road sort of machine, but as a vastly complicated organism in constant adjustment to an ever-changing environment. Involved in this adjustment is the kinesthetic return from multitudinous bodily attitudes, some of which are adequate (that is, in equilibrium) and some of which are not, but are calling for new and more satisfactory

[17] W. Köhler, Gestalt psychology, p. 84

[18] W. Köhler, Gestalt psychology, p. 106.

adjustments—tentative, overt, readjustive—until an equilibrium of response is set up, constituting a 'functional whole' that for the time being puts an end to the dynamic imbalance.

Addressing himself to the motor theorists of America, Köhler states his case by saying that "the kinesthesis occurring during (overt) movements is responsible for organization and grouping," and more specifically (to satisfy Miss Washburn and others) "mere *tendencies* to movements or faint reproductions of past kinesthetic experiences are sufficient to explain organization to other sensory experiences in the adult." [19]

As to the manner in which these factors produce a definite organization in the visual field, and in other areas of sensory experience, Köhler implies that with either overt or tentative movements, organization is mainly effected by some unique and mystic process. The answer of the attitudinalists, or motor theorists, toward this proposed explanation is that 'Organization' in the visual field, the olfactory field, the tactual field, and every other sensory field, is effected through tentative or overt motor adjustments which settle down to final equilibrium, a real *Gestalt*, if you please. Köhler declares that, in speaking further of the miracle-working 'Organization,' in psychology the right formula is "Constellation of stimuli—organization—reaction to results of organization." [20] For the motor theorist the order of events is "stimulation—tentative reaction—perception and organization—overt reaction." On the latter view it is possible to know what organization consists in, while for the former view, 'Organization' (with a capital initial) is still the elusive ghost we have been seeking to materialize.

In Köhler's direct attempt at an adequate statement of the character of a *Gestalt*, as was pointed out in the introduction to this article, namely, 'any segregated whole,' we are told that "the same general type of dynamical process which leads to the formulation and segregation of extended

[19] W. Köhler, Gestalt psychology, p. 166.
[20] W. Köhler, Gestalt psychology, p. 180.

wholes will also explain their specific properties . . . whenever a process dynamically distributes and regulates itself, determined by the actual situation in a whole field, this process is said to follow principles of *Gestalt-theorie*." [21]

As a matter of fact, when we look carefully through Köhler's writings, only one 'property' of an 'organized whole' is considered, and that is 'form.' The discussion turns on what is really the perception of segregated features of an otherwise seen whole. All this discussion in turn goes back to the original Gestaltian experiment of the *Phi-Phenomenon.* This is the experiment in which an intermittently projected beam of light plays at a certain rate now on one retinal area and now on another. Under certain conditions of rate and area, the observer perceives the light as actually moving from one of the spots to another.

To the Gestaltist, this phenomenon is one of the evidences of the existence of a mystic 'organized whole.' To the motor theorist it is the tentative movements that would accompany the actual shifting of a light back and forth on the screen if such a light were actually moving. It is not likely, however, that this phenomenon would arise for one who has never seen a moving light such as a glowing end of a stick swung about in darkness. Indeed, the phi-phenomenon can be made to disappear if one persistently perceives that it arises from discrete retinal excitations, as may be verified by trial by most observers.

When this same motor theory is applied to the illustrations of the *Phi-Phenomena* not only in the field of vision but of hearing and of tappings of the skin and to the multitudinous forms of shifting perceptions such as characterize the cube and staircase phenomena, the whole mystery clears up and the ghost ceases to be a phantom. The much discussed figure-and-ground idea of Gestaltists is, according to Miss Washburn, to be explained not by a mysterious 'Organization' but by the observer's tendency to take the tentative attitude of picking up the 'figure' but not the 'ground,' as has been explained. When the 'figure and ground' reverse them-

[21] W. Köhler, Gestalt psychology, p. 193.

selves, the ground is the part that one has the greater tendency to pick up, and the 'figure' is unreacted to at the moment, or at least the reaction to it is comparatively slight.

When an auditor listens to a pianist and the rippling of his melodies, how does the auditor approximate the successive mental states of the pianist? For Köhler, "The experiences of the pianist are a picture of the corresponding processes in his brain, so far as organization is concerned. Innervation of the pianist's muscles occurs as something like a projection of that picture upon his muscles. . . . In the similar nervous system of the listener a new organization is built up depending on those relations existing among the acoustical stimuli. To some degree it will be similar to that which exists or has just existed in the nervous system of the pianist." [22]

For motor theorists this mental 'behavior' stands thus: the pianist is stimulated by the page of printed notes before him to incipient adjustments which yield him the consciousness of the rapidly succeeding notes. As these incipient adjustments become overt, the notes are struck successively and arouse the successive incipient note-attitudes of the auditor, who in turn gets his auditory sensations and imagery from his own incipient attitudes, reenforced by contributions from his auditory zone. He may even imagine how the pianist feels toward his own performance by approximating the additional attitudes the pianist is taking as he sits at the piano.

In discussing the nature of association Köhler raises such questions as, "If five seconds after hearing a noise I hear a second one, similar but louder, how can I judge their relative intensities?" The answer is that "traces of past experiences are not an indifferent continuum of independent points; rather they must be *pictures of past organizations*. As such they take part in processes of reproduction." [23]

And what are 'pictures of past organization?' For the motor theorists, at least those who believe in the functional

[22] W. Köhler, Gestalt psychology, p. 258.
[23] W. Köhler, Gestalt psychology, p. 278.

value of consciousness, each of two successive experiences is associated in motor fashion with an individual's 'central activity system' or core of his behaving personality. Each impinges on some *tertium quid*, a third something, where the comparison is effected. How? The explanation will depend on one's theory of consciousness; but into these theories we do not go.

As to the laws of association we are told that, "if two processes, A and B, have occurred frequently together, reactivation of the trace of A is said to reactivate B in consequence of a curious bond between them created by these past events." But effective association best follows 'intentional learning,' which means 'intentional organizing.' But what the 'curious bond,' or the 'intentional organizing' may be, we are again left in wonderment. "Our conclusion," says Köhler, "is that association depends upon organization, because association is just an after-effect of an organized process." [24]

But the motor theorist is not left unsatisfied, for he recognizes some sort of organic tracery left in conduction tracts from which comes the law; "All association is association of movements," and it is movement systems that get 'organized' and inter-connected. This view of things offers an explanation of a phenomenon otherwise difficult to account for, that is, association of similars. What happens here, the motor theorist will say, is that two movement systems have so many conduction tracts excited in common that one system easily throws the other into incipient functioning, thus bringing into conjunction two responses that have never been actually associated contiguously in experience.

Quite the most puzzling of Köhler's conceptions is the one he calls 'insight.' It takes the reader some time to discover that the 'insight' he seems to have in mind is not the one the reader naturally brings with him to the discussion and upon which he eagerly welcomes additional light. After reading and re-reading back and forth over the pages of description one finally gets the insight (the usual term for the experience

[24] W. Köhler, Gestalt psychology, p. 299.

of going suddenly from a baffling problem to its solution—the 'aha-phenomenon') that what Köhler means by insight is found in the following statement:

"A great deal of purely sensory organization may be called 'silent' because, though we experience the *result* of it as segregated wholes with specific properties, we do not usually feel how this result is dynamically brought about and maintained, and in this respect the total field is different; it tells us more about its innermost nature. Here not only the result is experienced, but also very much of its why and how is felt in just the actual context. Whenever this is the case we apply the term 'insight.'" [25]

Otherwise stated, Köhler's 'Insight' seems to be awareness of the background out of which a behavior consequence ensues at the movement of this resultant.

The motor theorists have no difficulty in both recognizing and accounting for the phenomenon which Köhler describes. For them it results from the emergence of the 'how' and the 'why' attitudes along with the attitude of adjustment to the objective or solution of a problem.

On the more significant problem of the nature of insight in the usual English sense, what seems to happen when one is baffled and the proper solution suddenly presents itself, is that various tentative and trial responses to a given situation succeed each other until an adequate adjustment results. Sometimes one loses one's sense of direction or orientation, only to have the consciousness of the true direction suddenly 'click into position.' What 'clicks' is a satisfactory bodily adjustment, an equilibrium of tensions. Again, when a concept is formed out of a series of percepts, insight results when the elements of attitudes *common* to the individuals of a group are through sheer repetition made to stand out more conspicuously by becoming more tense. The process works similarly in all other cases of insight as popularly understood.

It is only fair to say that in Köhler's more recent writings there are more signs than in the earlier that he is moving toward a motor theory of consciousness, though he seems yet

[25] W. Köhler, Gestalt psychology, p. 371.

to have a long way to go. In a recent discussion of learning and insight,[26] a mysterious something is recognized as operative in both apes and men. As a working hypothesis, why not assume that in both creatures there is an incipient attitude toward a final objective, and as successive overt adjustments ensue in series, these cover at last the one that fits the incipient final adjustment when suddenly an 'aha-phenomenon' occurs?

What then, at last, is a *Gestalt?* For the Gestaltists, it is a dynamic organization, a balanced but undefined adjustment toward a situation. For the motor theorist it is a consciousness (mainly kinesthetic) of an adequate incipient motor response to a situation. More specifically, a *Gestalt* is a percept or a concept. But what are these, according to the motor theory of consciousness?

A percept is a mental state resulting from the arousal of the appropriate incipient motor attitudes toward a particular object. A concept is a mental state resulting from the arousal of the *common elements* of incipient motor attitudes appropriate to the known individuals of a class or group of objects.

To materialize the Gestaltian ghost and thus transform it into a very real and very useful member of the psychological family, all that is necessary is to convert a *Gestalt* into an equilibrium of tentative motor adjustments or initial excitations of movement systems. When a configuration, or form, or adjusted pattern of behavior is thus reduced to some such tangible elements as definite motor responses, the materialization of the Gestaltian ghost may be held to be complete.

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[26] Psychologies of 1930, pp. 143–160.

A BEHAVIORISTIC INTERPRETATION OF INTELLIGENCE

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The intelligent individual is the one who behaves in accordance with the accurate prediction of future results or conditions. The degree of intelligence varies with the accuracy of prediction and the amount of behavior which is guided by it. The act of predicting is more frequently known as 'purposing' and the behavior which is guided (or stimulated) by it is designated as 'purposive behavior.' In naïve parlance, intelligent behavior is usually identified with purposive behavior. We shall now attempt to give this interpretation an objective explanation.

The behaviorist denies that purpose, or intelligence, is the name for any non-behavior entity, or phenomenon, or even that it is a type of behavior which transcends mechanical explanation. Rather he believes that these words refer to a type of behavior which is antecedent to a series of subsequent responses.[1] In a series of responses, if the first part is of a predictive nature (*i.e.* if it describes a possible future condition) it may be isolated and designated as purpose.[2] For example, an individual may be stimulated to make the language statement (perhaps only implicit), 'I predict that if I study for the next two years at X University, I will be

[1] The writer disagrees with Professor Hull (*Psychol. Rev.*, 1930, 37, p. 511–25) that an organism can react "to an event which may be impending but which has not yet taken place." Suppose that the event does not take place. A series of effects would then be without cause. The law of the Conservation of Energy works only from the present to the future. In science, there is no such thing as an absent cause and in psychology, there is no such thing as an absent stimulus. (In case of the delayed response, the stimulus is present by proxy, that is, as neural after-effects or memory.)

[2] These are always responses of the language classification and may be stated in the indicative rather than the subjunctive mode. The indicative statement 'I am going to build a house,' is a prediction and may be stated 'I predict that I will build a house' or 'If I live long enough I will build a house.'

granted a doctor's degree,' (or 'I am going to study for two years at X University for a doctor's degree'). This verbal statement then becomes the determining stimulus for a long series of responses covering a period of two or three years time. During this time the behavior is called purposive. This simply means that some of the original responses of the degree-getting series were language responses of a predictive nature which then became determining stimuli for the rest of the series. The condition predicted by the original language responses (*i.e.* obtaining the doctor's degree) terminates the series and is usually referred to as 'goal.' Because of its importance as the terminal or effect of the series, the goal is confused by the teleologists with the cause. They say that the individual responds to the goal. However, the cause of a purposive behavior series is the language description of the goal which comes at the beginning of the series. The cause is the purposing or predicting, and the effect is the condition predicted or the goal.

The process of problem solving is essentially the same sort of behavior as purposing. The individual predicts the procedure (formulates a suggested solution) which will be necessary to solve the problem. He then tries it out, or tests it, and if he has predicted accurately, the problem becomes solved and the individual is classified as intelligent. If he has not predicted accurately, the solution fails and it is necessary to go through the same procedure again. Problem solving is a process of trying out suggested solutions or predictions. It is behavior which is in accordance with the prediction of future results. Purposing and problem solving are both types of behavior which are conventionally designated as intelligent.

There are a number of objections which have been raised to the mechanistic interpretation of intelligence, all of which are based on teleological assumptions. If we first assume that some behavior is teleological, it is then impossible to prove that intelligent behavior (or any other behavior) does not belong to this classification. But if we accept Lloyd Morgan's canon of science—"In no case may we interpret an action as

the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one of which stands lower in the psychological scale," [3]—the objections to our behavioristic interpretation of intelligence become vulnerable. Non-mechanistic explanation is of value only when dealing with assumed non-mechanistic phenomena. Such procedure may have value in philosophy but it is a hindrance in science. Scientific progress comes about only after teleological assumptions are abandoned.

A common teleological objection to the mechanistic interpretation of intelligence is that no machine, human or otherwise, can predict or describe a *future* condition. The behaviorist agrees with this objection but explains that the condition described in intelligent behavior is a past condition or a synthesis of past conditions. The actual future condition is never the condition described but *one like it.* How nearly the predicted condition is like the condition which actually takes place depends on the intelligence of the individual in predicting, and then on his ability to control the causes which bring about the future condition.

A second objection is that predictive behavior is not mechanical. For the time being, let us classify responses into those which are adequate adjustments to stimulating conditions, and those which are not. It is evident that purposive behavior falls entirely into the second classification. For example, Mr. B's office is cold. This stimulates him to close the window and turn on the heat. He has made a complete adjustment. A mechanical thermostat could do the same thing. But suppose that his office is cold and the *window is broken.* He can then turn on the heat and perhaps stuff paper in the window but the adjustment is not complete. The broken window is still there. So he makes the verbal response, 'I predict that if I tell the janitor, he will replace the broken window.' This stimulates him to call the janitor and complete the behavior series. Certainly this longer behavior series is no less mechanical than the shorter one. Both terminate in complete adjustment.

[3] C. L. Morgan, Introduction to comparative psychology, 1893.

Suppose that we construct a hypothetical calculating machine which will add only when the temperature of the room is 70 degrees Fahrenheit. (A thermostat would take care of this.) At other times the machine will automatically turn on an electric phonograph which will say, 'I predict that I will add those numbers when the temperature of the room becomes 70 degrees.' Then when the room is heated, the machine will automatically add the numbers and record the answer in the usual way. The calculating machine does not have a response adequate for conditions other than 70 degrees Fahrenheit. When it cannot make an adequate adjustment, it makes a 'purposive' statement, or it predicts future conditions. Its behavior is much less variable than human behavior but no more mechanical.

However, according to the teleologist, the problem is not yet solved. "Even if we should be able to equip the human body with all the reflex mechanisms imaginable, there would still remain to us the problem of making the reflexes combine appropriately so as to meet the exigencies of the particular occasion." [4] In other words, if man is a machine, it is up to the behaviorist to explain how he engages in the *proper* sequence of movements to terminate in the accomplishment of a goal. Suppose that a boy, who in the past has always had to climb a tree to secure apples, is stimulated to purpose to 'swipe' an apple from a grocery store. (He verbalizes, 'I predict that I can swipe that apple without being caught.') If he is a machine, will not the apple in the grocery store set off tree-climbing responses?

The behaviorist believes that an individual's behavior is determined by the following conditions: bodily equipment, stimulating conditions, physiological condition, and the after-effects of past behavior. [5] Let us assume that the bodily equipment and the physiological condition of the boy in the two situations (*i.e.* the apple-on-a-tree situation and the apple-in-a-grocery-store situation) are the same. He is hungry in both cases. Variation in behavior (in one case

[4] B. H. Bode, Conflicting psychologies of learning, Heath, 1929, p. 212.

[5] See Perrin and Klein, Psychology, Holt, 1926, p. 197.

approaching the tree, climbing the tree, reaching up for the apple, climbing down the tree, etc.; and in the other case slipping up, watching the grocery clerk closely, reaching down for the apple, running down the alley, etc.) must be explained by the after-effects of past behavior and the stimulating conditions.

The apples in the two situations could be essentially identical, but the other stimulating conditions are entirely different. In one situation there is an orchard, a tree, and no prohibitive conventions; in the other there is a store, a basket, a clerk, an alley, and prohibitive conventions. Except for the apple, the conditions of the two cases are entirely different and certainly would cause different behavior. The boy responds differently in the two situations because he is stimulated to respond differently. He always responds in the only way he can respond in that particular stimulus-response situation.

The sequence of responses which lead up to securing the apple, or the goal, is accounted for by the sequence of stimulation. With each response the boy is changing as a responding mechanism (the after-effects of past behavior), and with each response he is changing his own location in the stimulating situation.[6] If he is intelligent, he is approaching nearer and nearer to the condition described in the purpose verbalization. He constantly responds to the situation as it is and not as some other situation was at some other time. He does not make tree-climbing responses because there is no tree to stimulate such responses.

Another objection is raised by the *gestalt* psychologists.

[6] It must be kept in mind that the stimulus situation is a very complex affair. "Never does an organism respond to a single stimulus but always to complex stimuli situations. Hundreds of individual receptors are being affected constantly by hundreds of stimuli, both outside and inside the organism. The particular ones which are dominant vary from time to time but never are the dominant stimuli independent of other stimuli. Never are some of the organism's receptors entirely functionless while others are functioning. The ears are still hearing even though visual stimuli may be dominant. Certainly it cannot be said that the auditory stimuli are entirely impotent. A particular stimulus or group of stimuli are always within a stimulus situation, some of which is located inside the organism." J. S. Gray, A biological view of behavior modification, *J. Educ. Psychol.*, 1932, **23**, in press.

Professor Koehler observed, in his famous experiments on the Island of Tenerife, that his apes frequently solved long perplexing problems suddenly. The suddenness, he believed, indicated that the behavior involved 'insight.' It is difficult to understand just what Dr. Koehler means by insight for he gives a very complicated and intricate explanation.[7] However, insight seems to be that type of behavior which is in response not only to stimuli as such but to the *relationship between stimuli*. When the ape responded to the bananas as such and to the jointed sticks as such, there was no insight. But when he responded to the jointed sticks in relation to or as a means of getting the bananas, there was insight.[8] Insight exists when one stimulus is responded to as a *means* of responding in a certain way to another stimulus. The use of tools is a form of insight. One thing is used as a means to another goal. Stimuli are responded to in relationship. Now the gestaltists maintain that no machine, regardless of how complicated, can respond to relationships. A machine cannot respond to one stimulus as a means of enabling it to respond in a certain way to another.

Let us return to the hypothetical thermo-calculating machine. Suppose that we connect it to the heating system. When the temperature of the room is below 70 degrees, it will make a verbal prediction and then turn on the heat so that it can add the numbers and thus complete the response. It will thus respond to the heating system *in relation* to the temperature of the room and the numbers to be added. It is true that it can respond only to the specific relationship for which it has been constructed. But a greater variability of response would only necessitate greater mechanical complexity. It could be constructed so that it would respond to other relationships. Even humans are limited in the number of relationships to which they can respond. This number can be increased only by increasing the complexity of human structure.

Thus it would seem that the behaviorist is justified in

[7] W. Koehler, Gestalt Psychology, Liveright, 1929, Chap. 10.

[8] See W. Koehler, The mentality of apes, Harcourt, Brace, 1925, Chap. 4.

admitting purposive or intelligent behavior within his mechanistic system. Instead of being inconsistent with his mechanism it substantiates it at every point. Purposing is merely a distinctive type of mechanistic behavior and those individuals who can use it successfully are designated as intelligent.

From a social point of view, one of the greatest values of intelligent behavior is that it enables individuals to bring about the improvement of their living conditions. They are able to predict the type of condition which will be more satisfactory, and then to respond to these predictions (ideals) in such a manner as to bring about a close approximation of them. Let us observe a sample of this procedure. Mr. X is a smoker and has five pipes, all different and all with both good and bad qualities (physical and quantitative). Because of his language ability, Mr. X is able to discriminate between the good and bad qualities and respond to each in isolation. He is able to synthesize the good qualities into a word description of a pipe which is different from any of the five, yet is made up of qualities which he has abstracted from the five. He sends this word description to a pipe manufacturer and a new pipe is made which, presumably, is better than any which he now has. Note that Mr. X has 'created' something new. He has idealized (predicted) and then behaved accordingly.

Applied to the mundane conditions of life, there is theoretically no limit to the improvements which can be brought about by individuals behaving in that way known as intelligent. It has enabled humans to progress from those animal-living levels where conditions are all-powerful, to present civilized levels where man, to a limited degree, can control conditions. Because of the ability to behave intelligently, humans are now progressing in the direction of omnipotence. They are becoming able to control the conditions which control them. Certainly this is the goal of civilized development.

The particular value of a behavioristic interpretation of intelligence is that it eliminates those procedures based on non-scientific assumptions. The behaviorist does not waste

time trying to investigate a non-physical 'intellect' which is only supposed to be granted to each of us by a generous nature. He does not condemn an individual because he cannot find in him a 'spark of intelligence,' but believes that the individual has not yet been trained to behave in that way known as intelligent. The behaviorist deals only with physical data and never with entelechies. Consequently his investigations are less barren of reliable results than are those of the teleologist. A behavioristic interpretation of intelligence accelerates progress because it eliminates the useless effort of trying to investigate non-physical phenomena by physical methods.

To summarize: Intelligence can be verbally described as that type of behavior which is caused or directed by the accurate prediction of future conditions. Because of greater complexity of structure, and a consequent greater variability of behavior, a human machine can behave in accordance with the accurate prediction of a greater variety of conditions than can any other machine now known. However, there is no scientific evidence that human behavior is less mechanical than the behavior of non-human machines whether constructed out of protoplasm or inorganic material. Mechanism can adequately account for that type of behavior socially designated as intelligent except when non-physical phenomena are included in basic assumptions. Humans engage in intelligent behavior more than any of the less complicated machines, but to say that it is a characteristic only of humans is to transcend the limitations of Morgan's canon of science.

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A THEORY OF SERIAL LEARNING AND FORGETTING BASED UPON CONDITIONED REFLEX PRINCIPLES

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This paper is offered as a logical explanation, based upon conditioned reflex principles, of the characteristic and more or less constant form of the curve of forgetting. It is obvious that any theory of forgetting must draw upon some theory of learning; hence this development is fundamentally theory of learning.

The form of this curve is characterized by two major phases, *i.e.*, the abrupt initial drop and the following strikingly negative acceleration. In passing, the author wishes to call attention also to the seemingly paradoxical phase of the curve according to the results of Radossawljewitsch[1] (**8**). This phase, assuming that it is authentic, we shall consider as an extreme manifestation of the above-mentioned negative acceleration. Certainly it exemplifies negative acceleration from the viewpoint of forgetting.[2]

We shall be dealing with serial material and we shall represent the members of these series by the conventional symbols wherever possible. Further, we shall let the stimulus symbols represent visually presented nonsense syllables. Below is presented a key to the interpretation of the diagrams.

S Visual stimulus
R Vocal response

[1] The curve of forgetting from the results of Radossawljewitsch exhibits a rise following the initial drop.

[2] At many points in this treatment we shall lack experimental evidence. There will be many assumptions and implications which must be investigated and verified if this theory is to stand. However, the author believes these points to be of such nature as to lend themselves to objective investigation. Techniques for these investigations are being worked out, and these points shall be checked at the earliest possible date. To preserve the brevity and the theoretical aspect of this paper, these techniques are not presented.

s	Kinesthetic and auditory stimuli arising from R
>	Produces or gives rise to
$\Longrightarrow$	Previously established language habits
— – →	Immediate excitatory tendencies
——→	Remote excitatory tendencies
- - - -⊣	Inhibitory tendencies.

Now let us follow the establishment of tendencies or associations throughout the learning of a series of nonsense syllables barely to be learned; that is, to be learned to the point of one or two perfect reproductions. First, we have, at the beginning of practice, a situation wherein the subject simply responds vocally to the successively presented members of the series. This situation is represented diagrammatically in Fig. 1.[3] The visual stimuli S_1, S_2, etc., evoke respectively

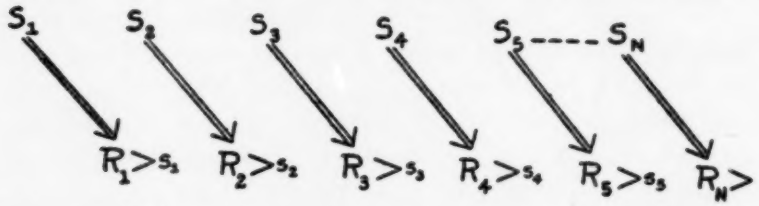

FIG. 1. Visual stimulus series, vocal response series and the auditory-kinesthetic stimulus series arising from the vocal response series.

the vocal responses R_1, R_2, etc. These responses are, of course, made possible by previously established language habits.

It is immediately to be noted that s_1 (kinesthetic and auditory) arising from R_1, and also S_1 (visual) immediately precede $S_2 \Longrightarrow R_2$. This relationship suggests that, following the redintegrative principle, the complex $\frac{S_1}{s_1}$ may acquire the ability to evoke R_2 without S_2 being present. In other words, $\frac{S_1}{s_1}$ becomes a conditioned stimulus for the response R_2. Likewise, $\frac{S_2}{s_2}$ and $\frac{S_2}{s_3}$ become the conditioned stimuli for R_3

[3] The arbitrary units chosen for stimuli and responses are conventional ones, selected from the necessity for simplification in the presentation of the theory.

and R_4 respectively, and so on throughout the series. Thus our subject, being stimulated by S_1 and having responded 'R_1,' may (being stimulated by s_1, auditory-kinesthetic) respond R_2 before S_2 is presented, and so on throughout the series. Further, when these responses precede their former visual stimuli they are reinforced by the appearance of the visual stimuli. When this becomes possible throughout one or two successive, perfect trials we shall arbitrarily say that the subject has barely learned the series. This situation is represented in Fig. 2.[4]

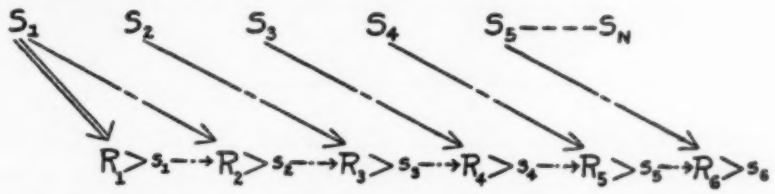

FIG. 2. Visual stimuli S_1, etc., with respective auditory-kinesthetic stimuli s_1 etc., constitute stimulus complexes serving to evoke vocal responses R_2, etc.

Now, for the sake of simplified diagrammatic representation, let us use but one component of each stimulus complex. S_1 has acquired the ability to evoke R_2 (we might quite as well use s_1— – →R_2), S_2 has acquired the ability to evoke R_3, etc.[5] This situation is represented in Fig. 3.

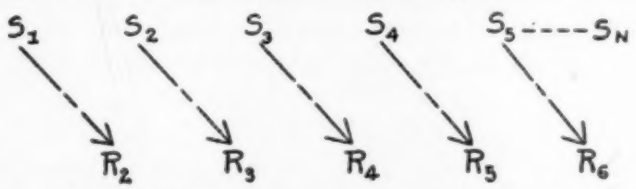

FIG. 3. Conditioned immediate, excitatory tendencies.

So far we have considered only the establishment of immediate excitatory tendencies. We have still to consider the establishment of the remote excitatory tendencies demonstrated by Ebbinghaus (**2**, 90–123) and by Hall (**3**, 65–76).

[4] This development of the establishment of immediate excitatory tendencies is essentially that of C. L. Hull (**4**, 511–513).

[5] S_1 is considered as being presented at the beginning of each trial, so, in each trial, S_1—→R_1 is nothing more than an old language habit.

Referring to Fig. 3, Ebbinghaus has demonstrated that S_1 tends not only to evoke R_2, but also tends to evoke R_3, R_4, etc., with progressively weaker excitatory tendencies. Likewise, S_2 tends also to evoke R_4, R_5, etc. These remote excitatory tendencies are added to our diagram and appear in Fig. 4, represented by single solid lines.

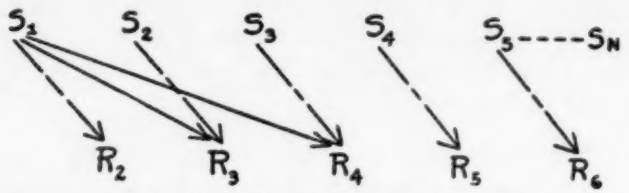

FIG. 4. Conditioned immediate and remote excitatory tendencies.

Now we may consider R_3, R_4, etc. as conditioned delayed responses to S_1; in other words, conditioned to a trace effect of S_1. There is no reason to presume a fundamental difference between delayed conditioning and trace conditioning. In each case the response is delayed, and in each case the conditioning is to the perseveration of stimulation, though in the former case it is a perseveration in the external world (as well as within the organism), while in the latter case it is a perseveration within the organism only. Pavlov (7, 40) assumed this organic perseveration. He (7, 88–105, 39–40) demonstrated the establishment of delayed and traced conditioned reflexes with his dogs. Referring again to the work of Pavlov (7, 48–49), he has demonstrated the internal inhibition or extinction of conditioned reflexes by lack of reinforcement. In the same experiments he has demonstrated the phenomenon of spontaneous recovery (7, 58–59). We have here, in our situation, the necessary conditions for the establishment of analogous tendencies, excitatory and inhibitory. When our subject responds 'R_2' to S_1 the immediately succeeding appearance of S_2 reinforces this conditioned immediate excitatory tendency. However, when our subject responds 'R_3' or 'R_4' to S_1 the tendency is not reinforced, for S_2 appears. This lack of reinforcement gives us the necessary conditions for the production of inhibition (internal inhibition

or extinction). Thus, as learning progresses, the remote excitatory tendencies are extinguished and we would expect a decreasing number of irrelevant intrusions or anticipations. This decrease in frequency has been evidenced in an experiment by Lumley (6). We now assume that when the series has been barely learned these extinctions are complete, and we have, as regards remote associations, all remote associations or remote excitatory tendencies held in check, so to speak, by inhibitory tendencies. In other words, all remote excitatory tendencies have been extinguished to or below a functional zero. This situation is represented in Fig. 5.

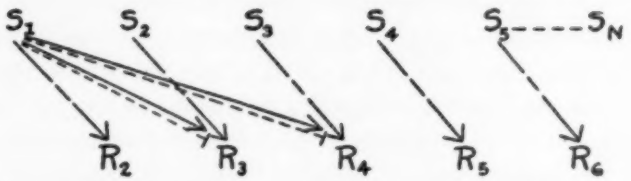

FIG. 5. Conditioned immediate and remote excitatory tendencies, and conditioned remote inhibitory tendencies in the form of internal inhibitions established by extinction.

The author wishes to suggest a possible alternative conception of the nature of the excitatory and inhibitory phases of these remote tendencies. It may well be that, instead of the complete extinction suggested above, we may have, established, in our learned series, phases of excitation and of inhibition analogous to the two phases of the delayed reflex as demonstrated by Pavlov (7, 88–105). With this view, the establishment of inhibitory tendencies, or phases, would again be accomplished by a lack of reinforcement occasioned by the appearance of the intervening syllable; and the establishment of the excitatory tendencies, or phases, would be accomplished by reinforcement after a period of delay, or, in other words, after the intervening syllable had come and gone. This possible alternative would not fundamentally alter our theory. This alternative does give somewhat more biological significance to remote excitatory tendencies. With this view, these tendencies would function to give added stability to learned series; stability which would be more or less temporary varying with the degree of overlearning. Also, this view would provide us with a logical explanation of the demonstrated advantage of rapid reading as compared with slower reading

in study. Briefly, the more rapid (within limits) the succession of stimuli, the shorter the periods of delay, or inhibitory phases, in the conditioned remote tendencies. Pavlov (7, 88–105) has shown that the stability of delayed reflexes varies inversely with the length of the period of delay. With this view, then, this would mean that a series learned by a rapid succession of stimuli would be more stable than a series learned by a less rapid succession of stimuli. These two implications are suggested to point out the significance of this possible alternative conception. The final choice between the two alternatives waits upon experimental evidence. The author has chosen to present the concept of complete extinction because it seems to offer the simpler explanation.

Now let us consider the 'inner nature' of our barely learned series as regards the presence and functioning of immediate and remote tendencies. We have first the functionally strong immediate excitatory tendencies $S_1 - - \rightarrow R_2$, etc. We have, also, remote excitatory tendencies which have been extinguished to or below a functional zero (internally inhibited).

Now that the above 'inner nature' has been established by barely learning our series, we cease practice and the period of forgetting begins. What happens to this 'inner nature'? Obviously, all established tendencies become weaker; the immediate excitatory tendencies, the remote excitatory tendencies and the inhibitory tendencies which hold in check, so to speak, the remote excitatory tendencies. There is no reason to believe that the remote excitatory tendencies weaken at a rate differing from the rate at which the immediate excitatory tendencies weaken. This is not true of the remote inhibitory tendencies. Pavlov (7, 58–59) offers evidence that inhibitory tendencies are much less stable than are excitatory tendencies. The phenomenon of spontaneous recovery exemplifies this evidence. Thus, as forgetting progresses, these inhibitory tendencies weaken and disappear first, which obviously releases the remote excitatory tendencies more and more into conflict with the immediate excitatory tendencies (spontaneous recovery). It may well be that this spontaneous recovery may take the form of the 'creeping' tendency described by Hull (5, 498–511), and given, by him,

much significance, especially in the case of defense reactions. This period of weakening and release and conflict gives us the necessary conditions for the rapid initial drop in the curve of forgetting. The author has purposely omitted the consideration of backward associations or excitatory tendencies. They complicate the theory, to be sure; but they offer no special difficulty. We would assume that backward excitatory tendencies (if they be valid) are extinguished and spontaneously recover in a manner no different from the extinction and recovery of remote, forward excitatory tendencies.

Now this period of conflict persists only as long as the recovered, remote, excitatory tendencies remain functional. These remote excitatory tendencies, being originally weaker, weaken and disappear after a relatively short period. This leaves the immediate excitatory tendencies functionally free, and thus relatively strong; perhaps, in some cases, even stronger than during the period of conflict (thus accounting for the rise in the curve of Radossawljewitsch). This weakening and disappearance and freeing from conflict gives us the necessary conditions for the abruptly negatively accelerated phase of the curve of forgetting. In other words, there remain now only the strong immediate excitatory tendencies or associations, free from conflict, to weaken, or to be forgotten at a slow and more or less uniform rate; again, the conditions necessary for the occurrence of the negatively accelerated lower end of the curve.

Some Phenomena for Which This Theory Offers Explanation

The author offers the following examples to suggest the possibly far-reaching implications of the theory presented in this paper.

1. This theory offers a new interpretation of the results of Hall (**3**) who attempted to reconcile the seemingly contradictory results of Ebbinghaus (**2**, 90–123) and Cason (**1**, 299–324). Ebbinghaus tested for remote forward associations after a twenty-four hour period of forgetting and gained

positive results. Cason tested for remote, forward associations immediately after learning with negative results. Hall tested for remote, forward associations, both immediately and after a week's period of forgetting, with negative results and positive results respectively; thus confirming the results of both men. The author's interpretation is that remote associations are extinguished in the learned series and become functional, and thus detectable, only after a period of forgetting during which they spontaneously recover. The author further suggests that the overlearning in Cason's technique extinguished the remote associations below zero, thus making negative results still more certain.

2. Lumley (**6**) has shown that irrelevant intrusions or anticipations in serial learning decrease as learning progresses. The author interprets these results as evidence of the extinction of remote associations or remote excitatory tendencies.

Lumley (**6**) has shown further that these anticipations are eliminated in an order related to the degree of anticipation; that is, the greater the degree of anticipation the more rapid the elimination. To illustrate, the anticipatory response R_5 to S_1 is eliminated before the anticipatory response R_3 to S_1. This means, to the author, that the remote excitatory tendency $S_1 \longrightarrow R_5$ is originally weaker and thus more rapidly extinguished than the remote excitatory tendency $S_1 \longrightarrow R_3$.

3. Without going into detail the author offers this theory as the explanation underlying William James' classic remark that we learn to skate in the summer and to swim in the winter.

4. The theory suggests the explanation of the oft-demonstrated advantage of distributed practice as compared with massed practice. Suppose we space our practice periods so as to avoid those forgetting periods characterized by the conflict between immediate and remote excitatory tendencies suggested in the above theory.

Summary

The above presentation is essentially a theory of forgetting based upon theory of learning, which in turn is based upon conditioned reflex principles. In this development the following assumptions have been made. (1) It has been assumed that the remote forward associations demonstrated by Ebbinghaus have the nature of delayed conditioned reflexes. (2) It has been assumed that these remote associations are extinguished, or possibly more stably established as delayed conditioned excitatory tendencies, during learning. (3) It has been assumed that these extinguished or delayed excitatory tendencies spontaneously recover from their total or partial inhibition after a relatively brief period of forgetting, and thus into conflict with immediate excitatory tendencies. We thus account for the rapid initial drop in the curve of forgetting. (4) It has been assumed that these conflicting, remote, excitatory tendencies, being originally weaker than the immediate excitatory tendencies, weaken and disappear first, after another relatively brief period of forgetting; thus leaving the relatively strong immediate excitatory tendencies functionally free, to weaken or to be forgotten at a more or less uniform rate. We thus account for the negatively accelerated, lower end of the curve.

This hypothesis, if it should prove to be sound, will serve to relate the phenomena of conditioned reflexes to the phenomena of habit formation, a relationship often assumed, but ill-defined, by contemporary stimulus-response psychologists; and, fortunately, it is of an accessible nature as regards testing by experimentation.

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[MS. received October 22, 1931]

DISCUSSION OF

"THE ADRENAL CORTEX AND EMOTION"

In a recent article (1) Hollingshead and Barton try to correlate the action of the adrenal cortex and emotion. Judging by their references, their conclusions are based on two lines of evidence.

Hammett (2, 3), who investigated the non-protein nitrogen (hereafter N.P.N.) in a small number of cases, showed that there is a greater variation in these blood constituents in people with marked emotional response than in those who are less susceptible to disturbing stimulation.

In seeking a cause for this variation, they use the work of Wyman and Walker (4, 5). These authors, after surgically removing the adrenal glands in rats, found a fall in blood sugar, and a rise in the N.P.N. (particularly urea) in the blood. This change is due to the cortex of the gland.

From these two lines of evidence Hollingshead and Barton suggest the following theory: that the adrenal cortical hormone is responsible for the variation in N.P.N. of the blood, and that it acts as an excitatory agent in emotional states. This cortical portion 'clears the blood of impurities.' "The reduction in the quantity of non-protein nitrogen would enable the blood to remove more rapidly the waste products of muscle tissues." This hypothesis is supported by the following consideration. As an excitatory hormone it should, in periods of excitement, produce a fall in N.P.N. figures. When the hormone is absent or diminished in quantity, the N.P.N. figures should rise. By analyzing the figures of Hammett, they find that in people characterized as excitable there is a low mean value for nitrogenous materials, and a high variability. In calmer people there is a higher mean value, with less variability.

The theory assumes explicitly that the cortical portion is called into action simultaneously with the adrenal medulla.

Against this theory the following arguments may be urged:

(1) There is no evidence that a given N.P.N. level in the blood is either a cause or effect, or is causally related with emotional states. From Hammett's work we conclude that there is a correlation between emotional stability and metabolic stability. But this is no

ground for correlating a given emotional state and a given N.P.N. level, such as the theory under criticism requires. Without such specific correlation the theory must fall to the ground. Experimental investigation of this point would be very simple, requiring only N.P.N. determinations before and during emotional excitement, but to my knowledge such work has never been done.

Further, in view of the wide normal variations of the blood N.P.N. (25–40 mg. percent), and its relation to the diet (exogenous nitrogen metabolism), the authors of this theory must attribute significance apart from dietary factors to such variation, a task which no worker on metabolism or kidney function has yet done, to my knowledge.

(2) The N.P.N. level in the blood is a resultant of protein metabolism on the one hand and kidney excretion on the other. In muscular activity there is, under normal conditions, "practically no change" (6) in the nitrogen output. There is thus no evidence that the N.P.N. level would be significantly raised, the exigency for which this theory prepares. There is no advantage to be gained by lowering the N.P.N. level in normal blood. The phrase 'clearing the blood of impurities' is thus meaningless.

(3) N.P.N. is removed from the blood by the kidneys. Rise in the N.P.N. level in the blood beyond normal limits is considered an indication of kidney damage. Such pathological increases, with or without emotional counterparts, must find a place in the theory under discussion.

(4) That the adrenal cortex exerts some action on the kidneys is quite probable, but the nature of its action is as yet unknown. It is an organ essential to life, its activity is protean, and its dysfunctions, as in tumors or Addison's disease, show a multitude of symptoms and signs, unnecessary to go into at present. To neglect all other functions, and to dispel the cloud of mystery by a bold assertion concerning non-protein nitrogen, is scientifically not wise.

(5) The authors present no evidence of their own but rely on the work of other men. When some of these other men flatly contradict the theory in question, that theory cannot be considered of great value. Thus, Hollingshead and Barton say, "The medullary portion (of the adrenal gland) . . . and the cortical portion . . . would be called into action simultaneously." But Wyman and Walker (5) say, "Much of the recent information concerning the physiology of the suprarenal cortex points towards a steady maintainance of certain bodily conditions by some influence from that

organ. By a steady maintainance it is implied that the influence from the cortex is constantly acting, possibly requiring appreciable time to produce its effects, and is not a factor which lies in reserve ready to be called upon."

In these criticisms no mention has been made of recent work in the field, especially the preparation by Swingle and Pfiffner of potent adrenal cortex extract. It might further be pointed out that the theory in question is of such a nature that it could easily have been put to experimental test. Some excuse for the failure so to have done should be forthcoming.

Conclusion: The theory of Hollingshead and Barton concerning the adrenal cortex and emotion cannot be called false, for only a rash man calls any scientific theory false. But this paper has tried to show that the hypothesis as stated does not follow from the grounds on which it is based; that it is rendered improbable by other relevant data not considered by the authors; and that no valid evidence in its support has yet been adduced.

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